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ART. I.—*Remarks on the Cause and Treatment of Cholera Infantum.* By L. P. YANDELL, M. D., Professor of Physiology and Pathological Anatomy in the University of Louisville.

Cholera Infantum has attracted much of the attention of medical writers in our country, as it seems, indeed, to be a disease almost peculiar to the climate of the United States. In 1794, Dr. Rush published an interesting account of the disease, which was called in Philadelphia the "vomiting and purging of children," and in Charleston, "the April and May disorder." The description of the complaint by Dr. Rush* is one of the most graphic and faithful ever given. Dr. Edward Miller, one of the editors of the *Medical Repository*, published in the first volume of that work a valuable article on the same subject—

* See *Medical Inquiries*, vol. 1, p. 159, 1st Ed.

"the cholera or bilious diarrhœa of infants," as he styled the affection. The medical journals of our country have abounded in dissertations on the subject since. It is proposed to add one more to the number; for although cholera infantum is by no means the fatal disease it was twenty-five years ago, it is still a complaint much dreaded, a source of great suffering, and of a fearful mortality among children.

It is the observation of physicians who have been long engaged in practice, that the "summer complaint of infants" has greatly abated within the period referred to. It seems to have been on the decline since the disappearance of epidemic cholera from our country, in 1835, and has certainly been much less prevalent during the last ten or fifteen years. The circumstance is an interesting one, that in the same period bilious remittent fever has prevailed far less extensively than it did twenty-five years before. Within the last fifteen years we have seen this fever, to a very considerable extent, superseded by typhoid fever. Next to bilious remittent fever, Dr. Harrison remarked, in 1828, cholera infantum required most the attention of Louisville practitioners; and before that time Dr. Meigs had stated, that one-third of the deaths among children in Philadelphia were occasioned by this disease alone. Dr. Joseph Parrish declared, in 1826, that "no disease contributed so largely to swell the bills of mortality in large cities, during its prevalence," as the one under consideration. "Were it not restricted to the summer season," he continues, "it would prove a greater scourge to the community than consumption itself."*

The fact of the decline of cholera infantum with bilious fever, doubtless has its significance. The relationship of the two diseases was early recognised. Dr. Rush observed that a fever of the "remitting kind," with "evident

* North American Med. and Surg. Jour. vol. 2, p. 68.

exacerbations, especially in the evening," attended upon cases of cholera infantum. Cleghorn, in his work on "Diseases of Minorca," had previously made the same observation. Dr. Edward Miller regarded the cholera of infants and bilious fever as branches of a common stock. But this affinity has not been universally admitted by medical authors. Dr. Parrish, in the paper referred to, ascribes cholera infantum to the agency of heat alone, unaided by miasms; and not a few writers have attributed it, with the common people, to worms, summer fruits, and dentition.

The question is a practical one. Our conception of the etiology of the disease will necessarily influence our method of cure. If there be a real connection between the complaint and malaria, it is important to be assured of the fact. This is the chief point of the present paper; and it is esteemed one of moment for the reason, that, if established, it must lead to a more efficacious method of *treating cholera infantum*.

Diarrhœa in children, there can be no doubt, is frequently excited by teething, and by indigestible food, whether fruits or other articles, just as similar causes give rise to disordered bowels in adults. Such disorders may be very severe, destroying life in a few hours, as cholera morbus is known to do; but most generally they are mild, and may be cured by an opiate, or a single dose of calomel. They, probably, do not come under the head of cholera infantum; they are unattended by fever, and do not recur after the patient has once been relieved. They are liable to occur at all seasons and in all places. Cholera infantum is eminently a relapsing disease. "We may obviate the more violent symptoms," says Dr. Parrish,* "we may procure temporary relief; we may even flatter ourselves that a cure has been effected; but the original causes have not lost their power; an increased susceptibility to their

* Op. cit. p. 68.

operation remains; relapse upon relapse is experienced; and at last the little sufferer, worn out by the successive attacks, sinks beyond the reach of medicine and expires."

The impression prevailed with some of the earlier writers on cholera infantum, that it is especially a disease of cities; and hence the advice of Dr. Rush and of Dr. Parrish to the parents of the little patients, to remove them, if possible, to the country. But it is well known at the present time, that children in some parts of the country suffer with the complaint as severely as do those in the city. Where benefit was derived from change of air, it was in cases where sufferers were taken from a malarious to a healthy atmosphere. This was the case in the neighborhood of Philadelphia; but the environs of our city are less healthy than the city itself, and no such beneficial effects are experienced here from the removal of sick children to the country. Patients with intermittent fever often get well on removing to a pure atmosphere.

Heat is generally regarded as the efficient cause of cholera infantum; the common name for the disease is "the *summer complaint*." Dr. Rush affirms that "its frequency and danger are always in proportion to the heat of the weather." Dr. Harrison makes the same statement. Is it true, that there is this exact correspondence between the complaint and the temperature of the weather? The fact that the disease has been declining for several years, while the summers have been as hot as ever they were, disproves such a supposition. The experience of the present summer is full in the face of it; we never passed through so hot a June in this region of Kentucky, and, on all sides, it is admitted, that there was never less cholera known among infants in that month, than during the last. "Although it is unquestionably true," says Dr. Cooke,* "that hot weather is the season in which cholera infantum is epidemic, it is to be observed that this disease

* Transylvania Journal, vol, 1, p. 192, 1828.

is not always most prevalent in the hottest summers. The summer of 1821 was, in the upper part of Fauquier county in Virginia, after July, uncommonly hot and dry, insomuch that the grass and even the Indian corn was burnt up; it was, nevertheless, uncommonly healthy." Cleghorn, in the work already quoted, records a similar observation. The weather in Minorca had seldom been felt so excessively hot as in the summer of 1749, and the drought was such that the farmers, in some parts, "scarcely reaped as much corn as they had sown." But notwithstanding the excessive heat, he says, "in June and July there were some specimens of the summer diseases, but so few that they scarcely deserve to be called epidemical."

Heat alone, then, we may conclude, will not develope cholera infantum. When the hot weather was attended with rain, Cleghorn encountered cholera enough, and the "wettest," he says, "are the most sickly seasons." At the same time, we have to admit, the prevalence of this complaint does not appear to be governed entirely by the weather, for we have seen that while our seasons remain the same the disease with us has been for some time declining. All that seems to be fully made out is, that the circumstances under which bilious remittent and intermittent fever prevail are those which favor the prevalence of cholera infantum—that they belong to the same family of diseases. This is all that is important in the present inquiry.

If, as Dr. Edward Miller suggested, "the cholera of infants forms one branch of that large stock of diseases" of which remittent and intermittent fevers are another type, it follows that the treatment should conform to the same principles. But it has not done so. The therapeutics have not had proper reference to the etiology of the disorder. The minds of physicians have been quite too much occupied with a single symptom of the disease—the diarrhœa. Our remedies have been too exclusively directed to the disordered state of the stomach and bowels.

Dr. Miller, in the paper more than once alluded to, says, that one of the principal objects of his communication was to recommend "the trial of mercury, alone or combined with opium"—a practice suggested by the work of Clark on "Diseases of Hot Climates." It succeeded so completely, he remarks, in a case of infantile cholera, attended with dysenteric symptoms, that he soon extended the use of calomel to the bilious diarrhœa of children, and found it to answer as happily as before. Some of his medical friends made trial of it, and, as they assured him, "with singular advantage." From this time forward the mercurial treatment became popular in cholera infantum; and its efficacy has unquestionably been great. The experience of the profession is probably not more uniform as to any point, than the relief afforded by mercurial purging in cholera and diarrhœa. The substitution of bilious evacuations for the watery discharges that attend these diseases, is well known to afford the greatest relief. Children laboring under the summer complaint rarely fail to be benefitted by bilious purging. So soon as the character of their passages is changed, there is an improvement in all their symptoms. Calomel checks the disease, but too often fails to cure it, and it returns again and again.

In one of these relapsing cases, in the summer of 1841, I thought I observed a connection between the return of the diarrhœa and evening exacerbations of fever, and was led to prescribe sulphate of quinine with reference to the chill which was supposed to precede the fever. The effect was most salutary, and the little patient was soon effectually relieved of his troublesome disorder. Other cases have been treated since in the same way, and with results as satisfactory. Dr. Booth has lately recommended the practice, in an article published in the last volume of this Journal, having called the attention of the profession to it some years ago in the New York Medical Ga-

zette. My conviction is that the practice is valuable. Doubtless there are cases of bowel complaints in infants which do not call for this remedy; but in many, it is believed, periodicity will be found to characterize the disease. and in these there can be no question, that quinine is *the* remedy.

Children often suffer excessively in this disease from being denied cooling drinks. The thirst is intolerable; but from prejudice, or because the sufferer is not able to speak and make its wants known, water is not given. A few weeks ago, I saw a child affected with the complaint whose cries satisfied me that it was suffering from thirst. Its mother said that the same plaint had been kept up throughout the preceding night. Water was given, and it drank eagerly As its stomach was not disordered, the child was permitted to drink as much as it wanted, and its cries ceased. The diarrhœa was easily arrested by tincture of opium; quinine was administered, and the patient remains free from the the disorder.

In an other case, some years ago, the thirst was accompanied by high fever, and an irritable stomach. The water which was freely allowed, was vomited almost as soon as it was swallowed. This patient was immersed in a cool bath, and with the fever, which soon subsided, he was relieved of his nausea and tormenting thirst. In very many cases, ice has been prescribed when the stomach would not retain water, and with effects as gratifying as any that have ever been presented to the writer in the practice of physic. The cold bath, ice, and cold drinks, constitute remedies of inestimable value in the treatment of this complaint. It is singular that they are not in more universal use. Dr. Miller urged them strongly in his excellent paper, published nearly fifty years ago. "As the cholera of children," he says, "is a febrile disease, and the surface of the body often heated far beyond the proper point, it will be advisable to expose all such parts of the

skin as feel too warm to the hand, to a stream of cool air or *to bathe them in cool water*. Several times a day," he continues, "the patient should be washed in vinegar and water, salt and water, or water alone, by means of a sponge, as he lies in bed, with as little motion, disturbance, or fatigue, as possible." Dr. Dewees wrote, twenty-seven years ago—"We have several times seen the most prompt advantage result from exhibiting to the little sufferer very small pieces of ice, every few minutes. It seems more certainly to abate thirst, and at the same time tranquillize the stomach better than water."

Every physician who has ever made trial of this practice has witnessed the same result. The effect is always salutary. The strong demands of nature, and true therapeutics are here, at least, at one. That which the sufferer craves above all other things, is that which more than al other things besides will contribute to his relief. There are cases in which cold enemata, introduced very gently, exert a happy influence. These are such as show a dysenteric tendency, and are accompanied by much febrile reaction.

Although the opinion has been expressed in this paper, that the diet of children alone will not induce cholera infantum, their diet and drink are by no means matters o f secondary importance. They may become the exciting causes of the disorder. There can be no question that they are continually provoking disease in the digestive organs of the young, even more than of the old. The quality of the water affects the health of infants in an unequivocal manner. It is the united testimony of the profession of Nashville, that since the erection of water-works there the children of that city have suffered much less with bowel complaints, and the improvement is attributed to the substitution of river for limestone water.

But it was not my purpose to enter into details, whether as to the prevention or treatment of cholera infantum.

It has appeared to me that the *febrile* character of the disease has been too much overlooked, and that consequently remedies of the most undoubted efficacy in fevers have not had a fair trial in the cholera of infants. Under this impression these pages have been hastily written, and are now respectfully submitted to the profession.

Louisville, July 12th.

ART. II.—*A Memoir of Dr. Charles Caldwell.* By L. P. YANDELL, M. D.

Before this article meets the eye of our readers, it is probable that the most distant of them will have heard, through the public prints, of the death of the venerable Dr. Charles Caldwell, so long one of the most frequent contributors to this Journal, and for many years identified with western medical education. The melancholy event took place at his residence, in this city, on Saturday afternoon, the 9th of July.

Dr. Caldwell was not only one of the oldest, but one of the most distinguished men in our profession at the time of his decease. In many respects, he was a very remarkable man. In appearance he was imposing. His bearing and manners in society were striking and peculiar. He attained to an age far beyond the period vouchsafed to the most of men, and a very large proportion of his long life was spent actively in public. He became an author nearly sixty years ago, and few years have passed since without the appearance of something from his pen calculated to keep him before the public mind. Of all the honored names of our profession, no one is, perhaps, more familiar to western medical readers.

Dr. Caldwell was born in Caswell county, North Carolina, of Presbyterian parents, and received the best education that was afforded by that portion of his native

State. At nine years of age he was sent to school, and very soon gave proof of a superior mind. Among those who knew him at this early period and who, having been his school-fellows, retained a lively recollection of his youth, were the late Hon. George W. Campbell, of Tennessee, and Dr. James Blythe, who was for many years before his death his colleague in Transylvania University. They were younger than Dr. Caldwell by several years. They represented him to have been a lad of great energy and ambition, fond of his studies and a proficient in them. From his aspirations and the evidences of mind which he gave, it was easy to be foreseen that he was destined to make a man of note. The profession for which he first studied was Divinity, but at a later period he abandoned it for medicine. Most of his friends have heard him relate the circumstances under which he gave up his original purpose. After quitting the study of theology, he opened a grammar school, in Iredell county, and spent three years as a teacher of boys. He then commenced the study of medicine with Dr. Harris, of Salisbury, and having pursued it privately with that physician for eighteen months, removed to Philadelphia, where he might have the benefit of a public institution. He was in the habit of stating in his lectures, that he continued the study in Philadelphia for five years, and in that time hardly allowed himself one day of relaxation. From the first day that he entered school as a little boy up to a very advanced period of his life, there can be no doubt that he was a most faithful, assiduous student. His account of his habits while a student, and in fact through life, was that he spent from eighteen to twenty hours a day in active mental labor, devoting but four, or at most six hours to sleep.

Soon after graduating he began to publish. One of his first works was a translation of Blumenbach's Physiology, which was followed, some years after, by a translation of Sennac on Fevers. During his pupilage, yellow fever

prevailed in Philadelphia, and the origin and nature of the epidemic became the subject of a warm and protracted controversy, in which he took an active part. He contended with Dr. Rush that the disease was of domestic origin, but going farther than his preceptor, insisted that it was not contagious. With these views he urged the futility of quarantine regulations, and the importance of a strict attention to hygienic measures.

Dr. Caldwell manifested thus early a disposition to controversy, which he continued to exhibit all his life. Dr Samuel Stanhope Smith, President of Princeton College, published, in the year 1787, an essay on the "Unity of the Human Race," in which he attempted to explain on physical grounds the diversity of color and physiognomy of the several branches of the human family. Dr. Caldwell, whose studies had been much in the French school popular at that day, attacked Dr. Smith's essay with great boldness, and, as he always himself felt assured, with entire success. But however able his review of the arguments of his theological opponent, the controversy did not bring him the credit which he derived from his discussion of quarantine. On the contrary it subjected him to the suspicion of being unsound in his religious opinions, and created prejudices against him which he did not outlive.

He evinced from the first more taste for the philosophy than the practice of medicine, and was always known rather as a writer than a practitioner. At one time he edited in Philadelphia the Port Folio, and wrote articles on politics, literature, religion, and the current events of the day. He also wrote the biographical sketches of Delaplaine's Repository, and was author of a life of General Green. His addresses before the Philadelphia Medical Society were numerous, and he delivered many political and literary addresses which were published at the time. The subjects upon which his pen was employed, embraced in its wide range, history, poetry, fiction, medicine, hygi-

ene, medical jurisprudence, biography, physics, natural history, belles lettres, and ethnology. He edited Cullen's Practice of Physic, but showed himself most at home when advocating the "vitality of the blood," or attacking Dr. Rush's theory of the "unity of disease." This propensity to generalize—to discuss theories, rather than to multiply facts by engaging in research, gave him early in life a reputation which he never lost—that of being a speculative, visionary man.

His fine elocution naturally suggested to Dr. Caldwell, at an early period, the idea of becoming a public teacher and he was not long in rising to a high rank among the lecturers of Philadelphia. Notwithstanding the hostility of Dr. Rush, which by some means he excited, he always had numerous private pupils, who bore flattering testimony to his eloquence and ability as an instructor. He had become widely known in the Western States as one of the most gifted of the medical writers and teachers in Philadelphia, and when the medical faculty of Transylvania University was reorganized, in 1819, he was elected professor of the Institutes of Medicine. Of the part which he took in building up that school, I have had occasion to speak recently. It is not too much to affirm that he was the father of the first Western School of Medicine. His colleagues were younger men, and were then comparatively unknown. However great the reputation which some of them have since achieved, they did not at first give character to the enterprise, and could not draw to it many students. Dr. Caldwell came to Kentucky a ripe scholar, an experienced teacher, an author of wide celebrity; and it is safe to say that he was now, for the first time, in his proper element. It was in a medical school, as an expounder of the "philosophy of medicine," that he was especially fitted to shine.

His introductory address, delivered at the opening of the first session of the school, was admirably suited to the

occasion. It breathed a tone of unbounded confidence. The undertaking, he said, would succeed beyond the expectations of its most sanguine friends, and would confer great and manifold benefits upon the profession and the country. It would throw much new light upon the diseases of the Mississippi valley, and "lead to a more rational and successful treatment of them." By reducing the expenses of a medical education, it would enable persons in moderate circumstances, as well as the wealthy, to attain it. It would promote the study of our climate, and especially its influence in developing or modifying our diseases. And it would "elevate in the west, the standing and dignity of the medical profession, and give it more weight and influence in effecting its benevolent and praiseworthy purposes." The ardor with which he embarked in the enterprise was communicated to his colleagues and to the pupils who had assembled to listen to his inaugural address; and it was only a few years until he had the satisfaction of seeing the school which he had been so instrumental in founding in the backwoods crowded with students, not from the frontier country only, but from Virginia, the Carolinas, Georgia, and other Atlantic States. He devoted himself to the institution. He wrote about it; he traveled to solicit the means for furnishing it with a library and apparatus; he made appeals to the Legislature of Kentucky for an endowment. The money thus raised he applied in Europe himself to the purchase of an excellent library and other means of medical instruction.

It was precisely in such an enterprise as this that Dr. Caldwell's best qualities were most fully and favorably displayed. It brought out that energy and resolution in which he so far surpassed the majority of men. How much soever others might doubt, his confidence was unwavering; whatever might be the faltering of others, his industry was untiring. As a teacher he was not so much

esteemed as some of his less distinguished colleagues; but his eloquence, his varied learning, and versatile powers excited the admiration even of those who regarded his lectures as wanting in practical interest. There was an elevation in his style, a measured pomp and roundness in his periods, a clearness in the arrangement of his matter, a confidence of tone and manner in giving out his opinions, a fullness and aptness of illustration in his lectures, which was sure to captivate a part of his audience. In all the classes before which he appeared during his long career as a public teacher, certainly until towards the close of it, there was never one in which there were not a number of students who agreed in placing him at the very head of medical lecturers. The writer of this notice does not hesitate to acknowledge that when a pupil of the Transylvania school he was himself of this number. He can never forget the delight with which he followed there the lectures on the "Institutes of Medicine;" a pleasure which was only exceeded by that of hearing Dr. Holley on Mental Philosophy. An incident occurred during that winter which, as it exemplifies the devotion of some of Dr. Caldwell's pupils to him, and is otherwise illustrative of his character, will not be deemed out of place in this connection.

At the time referred to, Dr. Caldwell took great pleasure in public discussion. He had become a practised and skilful disputant in the Medical Society of Philadelphia, where he had been accustomed to meet Chapman, Cooper, and other adroit debaters. Dr. Drake had just entered the school, and his powers as a controversialist were unknown. He was invited to read a paper before the Lexington Medical Society, at which Dr. Caldwell was present. In the course of his dissertation on certain points in Physiology, Dr. Drake introduced the doctrine of Harvey—*omnia ex ovo;* that all living beings come from germs furnished by parents. The allusion to the doctrine

seemed to the audience generally to have been accidental. They could see nothing in it calculated to bring on a long and spirited controversy; but the author of the paper knew full well upon what ground he was entering. He had heard, when a student in Philadelphia several years before, an animated discussion between Dr. Caldwell and Dr. Chapman on the subject of "spontaneous generation," provoked by a paper read to the Medical Society by himself. He did not doubt that Dr. Caldwell was still a believer in that doctrine, and he as little doubted his readiness to support it. Dr. Drake had become aware that he was himself endowed with rare faculties as a debater, and was willing to try his str ngth with his learned colleague. As he expected, soon after he sat down Dr. Caldwell rose and addressed the society. He complimented the paper of Dr. Drake for its ingenuity, learning, and general ability. But there was one tenet, he said, to which he could not subscribe; and he went on to show the grounds upon which spontaneous generation appeared to him still a reasonable hypothesis. Dr. Drake replied, and Dr. Caldwell made a further exposition of the doctrine. It was resolved to continue the discussion at the next meeting of the Society.

It was resumed the ensuing week. In his remarks at the previous meeting, Dr. Caldwell had quoted from the writings of Moses and some of the fathers in medicine certain opinions or statements which seemed to favor the hypothesis he was advocating. This afforded Dr. Drake an apology for bringing forward all the written testimony he could find on the subject, and accordingly he appeared before the society, this evening, with a load of authorities. He piled quotation upon quotation. All the instances cited by his adversary as examples of equivocal generation, he showed, had been accounted for by the presence of germs. It is seldom that one ever listens to a more convincing argument than Dr. Drake made on

this occasion. It appeared to his audience conclusive, overwhelming. There was no replying to his facts; no escaping from their irresistible force. The whole medical class, numbering about two hundred, were with him, all but two. One of these was the writer of this sketch; the other was a friend who was also a Tennessean, now no more—George W. Campbell, whose uncle had been the schoolmate of Dr. Caldwell. It is possible that we felt the force of Dr. Drake's powerful argument as much as our classmates, but we could not be made to admit it; we stood resolutely by our favorite teacher. It is not often that a leader has had fewer adherents; but it made no difference with ours, who was impassive to the facts which convinced every body else, and to the last, held as stoutly to his cherished theory as his devoted young friends had done to him.

Dr. Caldwell was as faithful an officer as ever belonged to an institution of learning. He toiled unceasingly for the advancement of the Medical school at Lexington, and it was in the full conviction that its interests could no longer be promoted there, that he advocated its removal to Louisville. He had resisted the conviction as long as he could. After others had begun to talk of erecting a school in this city he wrote in favor of the advantages of Lexington. He defended the site as long as it appeared to him defensible, and with arguments, the soundness of which must have seemed to him questionable while using them. But when he became an advocate for the removal, he was an undisguised, unwavering advocate. He never changed his views, and never deviated from his purpose. He urged the policy openly, and on all proper occasions; and when at length convinced that the Lexington school could not be transferred, he was for establishing a new institution in Louisville.

There is no occasion for adverting in detail to the unpleasant controversy which grew out of the attempt to

transfer the Medical department of Transylvania University to Louisville, the history of which is familiar to most of the readers of this memoir. It is well known that Dr. Caldwell bore much more than his share of the censure, attached by the people of Lexington to the medical professors for conceiving and attempting to carry out such a project; and at least one of his colleagues was involved in the acrimonious discussion which followed by his efforts to shield him from the unmerited obloquy. But if he was unduly censured for his interference in behalf of the school at Lexington, he entitled himself, by his zealous efforts to rear an institution in this place, to be called the founder of the University of Louisville. Doubtless the University would have been founded without him, and perhaps founded very soon. The time for erecting a medical school in Louisville had arrived, and many minds were turned favorably towards the measure. But, at the same time, all will admit, that it was Dr. Caldwell who gave the impulse to it at the auspicious moment. He was the leading man in the movement, and his energy and perseverance did much towards securing the donation, and placing the institution upon an ample basis.

Dr. Caldwell was an active and very voluminous writer. He produced no elaborate work, but he was continually writing, now on phrenology, and then on the study of the ancient languages; at one time on mesmerism, and then again on animal chemistry. His pen was never idle. He must write and publish. It had become with him a fixed habit; it was his amusement, his aliment, his life. He was as much the founder of the medical journals as of the medical schools of Kentucky. It was by the dint of his importunity that the medical Faculty at Lexington were induced to project the *Transylvania Journal of Medicine and the Associate Sciences*, and to assure the editors of a supply of matter he pledged himself to contribute to every number. No one had a higher appreciation of the

salutary influence of such a periodical upon the profession. He foresaw how it would quicken the medical mind of the country, and what a harvest of experience would be gathered in by it; and it was therefore upon principle that he strove to have one commenced and sustained.—When Louisville became the theatre of his action, he was as zealous for the establishment of a journal here; nor did he rest until he accomplished his wishes. The *Louisville Journal of Medicine* was suspended after the issue of the second number, but he began at once to agitate for a renewal of the effort; and in due season the Journal which is now in the hands of the reader was set on foot. Dr. Caldwell did not edit any of these works, but in one sense they may be said to have originated with him; he was by far the warmest advocate for their projection, and for a long time the most prolific contributor to their pages.

A few years since Dr. Caldwell computed that his writings, up to that time, including translations, pamphlets, essays, and books on various subjects, would amount in the aggregate to more than ten thousand pages, and supposed that they might reach eleven thousand. Since that time he has published probably not less than a hundred pages in the shape of pamphlets and essays. He had then made two hundred and eleven distinct publications, of which six exceeded three hundred pages, thirteen were over two hundred pages in length, twenty-one over a hundred pages; four-fifths of the whole being under fifty pages. It is manifest that it was not by detached, ephemeral tracts, such as most of these, that a solid, enduring reputation was to be achieved. And then the subjects were not only greatly diversified—too numerous, it may be affirmed, to be thoroughly compassed by any human intellect; but many of them were unfortunate subjects Too many of them were topics that serious, philosophical minds have agreed in rejecting as unworthy of grave investigation; and upon too many more the

opinions which he entertained were at war with the science of the age. The greatest of his works, unquestionably, are the schools which he founded. Aside from his merely literary productions and his translations, much the larger portion of his writings relates to animal chemistry, phrenology, and mesmerism. When he touched upon questions pertaining to natural theology, he continually broached views which gave offence to the friends of revelation. On the physiological topics, about which he wrote most copiously—'solidism,' the 'humoral pathology,' 'sympathy,' animal chemistry'—he stood almost alone, vainly struggling against the onward progress of scientific discovery. Phrenology, and mesmerism, the cherished topics upon which he bestowed most time and labor, and which it was his glory to have been foremost in advancing, whatever may be their real merits, have not yet obtained a footing among the accredited sciences. It seems to have been a peculiarity of the mental constitution of this gifted man, that the truths which most thoughtful men regard as essential and best established, were those about which he doubted most, while the doctrines which scientiffc men generally condemn, were those which he most cordially embraced. The effect upon his reputation was unhappy while he lived, and cannot fail to be so upon the popularity of his writings.

His style, when young, was involved, inflated, diffuse, and inclined to bombast. In these respects it improved with age, without ever becoming easy, simple, or graceful; but it always possessed the qualities of vigor and clearness, and had the merit also of being peculiar. No one was ever at a loss for his meaning, or could be long in doubt as to his authorship. His remarkably fine personal appearance and bearing were not more distinctive than certain forms of expression which were sure to reappear in his writings and at once revealed their author. No author, perhaps, ever repeated himself so often; and

this gave a monotony to his productions, especially the later ones, which detracted very much from their interest.

The subject being given, it was easy for those who were familiar with his writings, to anticipate, not the drift and arrangement of his discourse merely, but the facts, the arguments, the illustrations, and even many of the very phrases. In conversation, in writing, and in lecturing he was the same; always forcible, earnest, and confident, generally elevated, never obscure. His mind was always full of his subject, and he never failed to make himself understood by his readers or hearers of whatever capacity. If he failed often to carry conviction to others, it was not because he had not the air of being himself fully persuaded of the truth of what he urged.

Of all his professional cotemporaries in this country, Dr. Caldwell had the highest opinion of the talents of the late Dr. Chapman. No medical man in America was deemed by him to possess so inventive and original a mind. There is something interesting in the circumstance, that these eminent men, who had lived as friends so long in the same city, and pursued together the same line of investigation, should have closed their earthly career so nearly at the same time. In the mould and texture of their minds, and in their general bearing, Dr. Caldwell and Dr. Chapman were not unlike each other. They were both finished scholars, and ornate, showy lecturers; they adopted a similar style of writing and declamation; their tastes inclined them to the same course of study, and they embraced many of the same unpopular doctrines in medicine. They were alike exclusive and extravigant in their views concerning the humoral pathology, sympathy, and the vital principle. The notions which Dr. Chapman early in life put forth in respect to the *modus operandi* of medicines, Dr. Caldwell was found strenuously supporting in some of the last papers he ever published. Dr. Chap-

man was the more practical man of the two, and being drawn by circumstances more towards the active duties of his profession, was saved from those purely speculative and visionary studies upon which his friend wasted so much of the vigor of his life. They rose to almost equal distinction. One was at the head of his profession at the East; the other was for a long time the acknowledged head in the West. They were both connected for many years with flourishing medical schools. Dr. Caldwell had not the readiness or the wit of Dr. Chapman, but he had more energy, and greater powers of application; he was better fitted for rearing an institution of learning in a new country. In both were united to minds of a high order talents for display which gave them great influence among men.

Among the associates of Dr. Caldwell in his long connection with the medical schools of Kentucky, the colleague whom he ranked highest for originality and vigor of mind was Dr. Drake, who also has preceded him to the grave. Dr. Drake always claimed to have been the the pupil of Dr. Caldwell, having attended his private lectures in Philadelphia, and was a younger man by a good many years. Their association in important enterprises during the most active period of their lives naturally suggests a comparison of the characters of these distinguished men. There were few points of resemblance between them. In manners, tastes, style of writing and speaking, habits of thought, social impulses, and medical and philosophical opinions, they differed the most widely from each other. Except minds of great compass, rare strength of will and energy of purpose, and an industry untiring in both, they possessed few qualities in common. Dr. Caldwell had the advantage of a s superior education, but Dr. Drake continued to be the greater reader, and so kept himself abreast of the tide of discovery in his profession and conformed his teachings

to it. Dr. Caldwell had great boldness, and launched his theories forth regardless of the opinions of other men. "Better occasionally broach startling errors than deal in time-beaten truisms which awaken no curiosity," was one of his published sentiments upon which he acted.—Dr. Drake was cautious in hazarding opinions which he formed after long investigation. His style of speaking was less finished and graceful than that of Dr. Caldwell, but it was more earnest and effective. His voice was more powerful, and the operation of his mind far more rapid and intense. He had a readiness, a keenness, a command of language and apt illustration, that rendered him formidable in debate, and quite an over-match for his more learned colleague. In manner and appearance one of the most diffident of men, when he engaged in a controversy he was one of the most assured. His wit, his readiness of repartee, his acuteness of analysis, delighted and astonished his audience. Dr. Caldwell excelled in a clear exposition of his subject; in the methodical arrangement of his positions; in the symmetry and order of his discourses, where there were none to controvert or object. Dr. Drake excelled in debate in which his wonderfully active mind was always ready with the ingenious argument, the pertinent fact, the happy retort. His style of writing was more simple and natural than Dr. Caldwell's, and therefore a better style for scientific subjects. In conversation, the difference between them was that Dr. Drake was unequal—sometimes dull and tiresome; sometimes singularly eloquent, original, brilliant, and instructive; while Dr. Caldwell always did justice to his reputation. In personal appearance Dr. Caldwell had the advantage. His head was one for the sculptor, as classical, as finely developed as phrenologist could desire to behold. His person was large, well-proportioned, stately and commanding. No one could see him without remarking him.

It has been remarked, that Dr. Caldwell's taste led him to the speculative in science. Dr. Drake inclined to the practical. When a young man, Dr. Caldwell gave lectures on natural history, but it was the natural history of books; he made no observations of his own; engaged in no original researches; and later in life he forsook these studies for psychology. Dr. Drake was always an observer, and from the earliest period to the close of his professional life was engaged in independent research. His first work—the *Picture of Cincinnati*—was a volume of facts and statistics; his last and great work—the *Diseases of the Interior Valley of North America*—was like it, a vast treasury of facts gathered up through numerous years and in the course of many long journeys. While Dr. Caldwell showed more perseverance in erecting medical schools, Dr. Drake showed greater steadiness of literary aim; and if the latter left behind him no successful institution to which he could point as of his creation, he produced a work of which Dr. Caldwell, among all his publications, has left behind him no equal.

Dr. Caldwell's excellence as a medical teacher lay chiefly in the faculty of inspiring his pupils with a love of science, and an ambition to excel, and in this he was eminent. His defects were those that belong to his writings;—a propensity to speculate; an undue preference for subjects of equivocal value; a proclivity to doctrines generally discarded. A more serious defect, which, whether it was engendered by the prejudices of early education or by associations formed later in life, is lamented by his friends as the capital one of his character, was the want of a sincere religious faith. This showed itself in whatever he did. It was continually appearing in his writings; it was remarked in his daily conversations; and he was unable to conceal it in his public lectures. It was keenly felt by himself; no one, probably, knows how keenly. "*I keep up a storm without to silence the storm with-*

in," he has been heard to remark of himself. He was aware how much of influence he had lost from this cause; but he could not have been aware how much it had to do in giving his mind those unfortunate scientific biases which the friends of his fame have to regret. With a mind more happily constituted in this respect, possessing a person so distinguished, talents so varied and so highly cultivated, so fine an elocution, such physical powers of endurance, and an energy so indomitable, it would be difficult to say to what height he might not have risen.

ART. III.—A *case af obstinate and prolonged rigidity, with spasmodic contraction of the neck of the Uterus during parturition, treated by the combined administration of Ergot and Chloroform.* By NAPOLEON B. ANDERSON, M. D., of Louisville, Ky.

During the past few months, as several cases similar to the one in question have come under my treatment, (only less obstinate both in duration and intensity, but in which the same agents were employed with the like good results,) I am disposed to publish the annexed case from the fact that similar circumstances have never come under my own observation where the adoption of these remedial agents was so applicable, and resulted in so precise an action as their administration was wont to fulfil.

Ergot, I can but suppose, as having no direct influence in the dilatation of the mouth of the uterus, its action being secondary, it is a mechanical pressure exerts through the agency of the fœtus—whilst chloroform has the power of effecting this object—and where there exists a stubborn condition of the neck towards dilatation, with inefficient pains, there does not exist more safe, though potent remedies, by which the difficulties can be overcome, than the timely and judicious use of ergot and chloroform, pro-

vided there exists a proportionate size of the pelvis, with proper relaxation of the soft parts; and when the administration of these agents does result unfavorably in their action, the consequences will be found in every case to be the result of unforseen barriers existing in the formation of the pelvis and soft parts, or the idiosyncrasy of the patient forbidding their use.

On Sunday morning, June 19th, I was in attendance on Mrs. Mary Finley, of this city, then in labor with her second child. She is twenty-four years of age, possessing an unusually healthy constitution, being well proportioned in every respect. On my arrival, I was informed that she had been in labor the previous twenty-four hours, the pains having been small but regular. On a vaginal examination it was found impossible to reach the mouth of the womb in almost any position of the patient, and it was only after repeated trials that it was brought down by elevating the hips high in bed. The position of the os uteri was found to be backward, upward, and to the left side, imparting to the touch a sensation similar to that produced when pressure is made on the cartilage of the end of the nose. At each successive pain it was thrown back in the same position, and it was found almost impossible to retain it in the axis of the pelvis. Finding it impossible to control the position of the neck, she was suffered to remain unexamined for several hours, during which interval the neck righted itself, but remained uninfluenced by the pains which were materially augmented. The extract of belladonna was freely used on the neck, which, in some five hours, produced but slight dilatation, and the remedy soon lost its influence, the parts remaining as hard and obstinate as in the commencement of labor. The pains soon became constant and expulsive, but were attended by no beneficial result, as the parts refused to yield to the hardest pains. The patient became flushed, excited, and experienced great pain in the head, for

which she was freely bled, but producing no beneficial results so far as dilatation of the mouth of the uterus was concerned. Antimony was then resorted to, and she was brought under its full influence, without the slightest effect. The finger was then made an agent by friction constantly kept up around the neck of the uterus until its influence produced slight dilatations, which soon, however, lost its effect. The lips were flabby, yet hard, and the stoutest pains seemed to have no direct influence whatever in accomplishing the requisite enlargement to enable the birth of the child.

After exhausting a great amount of her energy, the pains grew less frequent, feeble, and finally abandoned her almost entirely. She remained in this condition several hours, when the pains again recurred, though weak and regular; and as the soft parts were readily yielding, she was brought under the influence of ergot in 20 grain doses every twenty minutes, until she had taken one and a half drachms. She was soon under its full influence, but the womb positively refused to yield to the most regular and hardest pains, the parts rather seeming to contract.

Her critical condition prompted a resort to chloroform, the influence of which she was soon under, and whilst the pains seemed in no manner interrupted, dilatation immediately ensued, and after a few pains the child's head was born. The uterus instantly, however, contracted tightly around the neck of the fœtus, without the slightest cessation of the pains, and no manual manipulations, used with regard to the safety of the child or future health of the mother, could have assisted in the birth. The chloroform was again repeated, and resulted in the same happy effect.

Anticipating a sudden contraction upon the placenta after the birth of the child, the hand was immediately introduced into the uterus, whilst the attendants removed

the child. Yet, notwithstanding this precaution, the contractions were so severe and spasmodic as to require her to be kept under the influence of chloroform until its removal, which was accomplished with the utmost difficulty. The after-pains were prolonged and severe, and the lochia soon ceased to flow; the former was alleviated by anodynes, and the latter by emollient poultices combined with camphor. After the usual period, she again recovered, and now enjoys excellent health.

The above case, it is reasonable to suppose, had it not been for the ergot and chloroform, would have terminated in the death of the fœtus and probably the mother, or of both, as all the remedial agents generally used were faithfully put in requisition, with the exception of instrumental interference, which would have been criminal to have attempted. It may be questioned as to which of the agents the dilatation of the os uteri is due, or as to whether their combined influences were not the exciting cause. Be this as it may, I feel fully assured that had not ergot been given after the pains became inefficient, the birth would not have been accomplished but by the aid of instruments, the use of which, however, would have been out of the question in this case; and I am confident that but for the use of chloroform, preceded as its administration was by ergot, the consequences might have been serious to both mother and child, from the effects of a rigid undilating uterus. The case is clear, to my mind, as one of the remarkable influences and effects of these two agents, and I feel gratified at having been the means of their use in this case, as I am sure but for their administration, two more beings would have been numbered with those that have preceded us.

Louisville, July, 1853.

ART. IV.—*Hints with regard to the natural history and medicinal properties of Veratrum Viride.* By ISAAC BRANCH, M. D., of Abbeville, S. C.

This plant has recently become an important article of the materia medica, at least in the South, and it is for the purpose of introducing it into the North-west that we have consented to write this concise article. We have used this article extensively ourself, with the most satisfactory results, and believe that it is not only the privilege, but the imperious duty of every member of the profession to contribute his mite to the stock of general knowledge, so that the result of the experience of all may be placed in some tangible form before the professional world. Our remarks will be concise, and we trust to the point, and as they have been spread on several occasions before the profession in the South upon the same subject, we must be exceedingly brief on this occasion. Indeed, our peregrinations for the coming few months, will not permit us to write an extensive article. We will add that we have never sold an ounce of the article in our life, nor are we interested, in any manner whatever, in its sale.

At the instance and request of Dr. W. C. Norwood, of South Carolina, who is entitled to more credit for investigating this article than all of the rest of the profession combined, we have deposited, for him, a small quantity of the pure tincture in Nashville, Louisville, and Indianapolis. The formula for its preparation has been published to the world by Dr. Norwood, time and again, and for the informationof those who have not seen it, we will repeat it:

Root of Veratrum Viride, 1 lb.
Officinal Alcohol, 1 pint.

Macerate for about ten days, decant and strain, and it is fit for use. It may be thought that we use an unneces-

sary quantity of the root, but the object is to have a pure saturated tincture, and all we have to say in regard to its use and merits, contemplates a tincture of that kind, therefore, a little extra quantity of the root makes no difference, whilst a little short of the requisite quantity will materially affect the article. Any practitioner of medicine may prepare this article for use, provided he can procure a pure article of the root, and here rests the difficulty. If he orders the *Viride*, he is liable to procure the *Album*, and vice versa; indeed, the endorsement upon the package procured, affords no security; for the *Viride* is more apt to be labelled *Album* than otherwise. We will explain this confusion.

The *Veratrum Viride* is an American plant, whilst the *Veratrum Album* is exclusively and entirely European. Until comparatively speaking quite recently, they were regarded even by botanists as identical, and the same identical medicinal qualities ascribed to each. In external appearance they somewhat resemble each other, whilst in medicinal properties they differ widely.

We regard this article as about as important in the treatment of typhoid and other affiliated diseases, as the sulphate of quinine is in the treatment of intermittent and remittent fevers. We are aware that this is assigning to the article under consideration a very prominent position—it is placing it upon the top shelf, but our experience justifies the remark. Although we have never used this remedy in a pure case of typhus, indeed we have never seen such a case at the South, we believe, reasoning from analogy, that it is as well adapted to the treatment of typhus as it is to typhoid diseases. In any case of disease with a hot and dry skin, and a frequent pulse, we should most assuredly resort to its use, guarded by the views we shall advance in the sequel The article is peculiarly adapted to the treatment of asthenic diseases—it diminishes the *frequency* of the pulse, whilst

it does not reduce the *strength*; indeed, we have very often seen the strength considerably increased under its use.

Our plan for using the article in typhoid diseases is as follows, and we commence our treatment at our first interview with the patient: we never resort to any preparatory treatment. In a case of typhoid fever or typhoid pneumonia, presenting the ordinary symptoms, if the patient be an able-bodied, athletic adult, we commence the treatment of the case with from five to seven drops of the tincture, giving it at intervals of two hours, increasing a drop at each succeeding dose, until the pulse is reduced to the desired standard as to frequency, which will generally be the case in from eight to twelve hours, and here we hold the pulse with more or less of the article, giving it at regular intervals until the disease is broken up. We have often taken a case with a hot, dry and husky skin, a dry tongue, great restlessness, with a pulse of 140 in a minute, and within the course of twelve hours we have had a cool skin, a moist surface, a tranquil condition of the system, with a pulse of from 60 to 80 beats in a minute. The only bad effect which ever results from the use of the article, is excessive nausea, retching, and vomiting, and there is no difficulty in guarding against this, if small doses are used in the beginning of the treatment, and increased as above detailed. After the pulse is brought under the control of the article, three or four drops will frequently hold it there if repeated every two or three hours. There are cases of typhoid diseases which require anodynes from the commencement of the attack, in which cases we usually give a tea-spoonful, more or less, of paregoric, or an equivalent quantity of opium or morphine, in some other form, with each dose. We usually give a blue pill, or some other cecoprotic—we never purge.

If the patient be a feeble lady, or a child, we give less of the article, according to circumstances, until the susceptibility of the patient is ascertained. We have never used the article in scarlet fever, but can have no doubt of its beneficial effects in such cases. We ask the profession in the Northwest to try it. We have not time to continue our remarks.

REVIEWS.

Art. V.—A *Treatise on Lightning Conductors;* compiled from a work on Thunderstorms, by S. W. Harris, F. R. S., and other standard authors. By Lucius Lyon, A. M. New York: George P. Putnam. 1853.

(Concluded.)

In a notice of this valuable work, in the July number of this Journal, we endeavored to develope some of the curious facts connected with electrical phenomena. We pass now to the principles concerned in the metallic conductors. The remarks on these laws are invaluable, and we make liberal extracts. The author says:

"The heating effect of the same, or different quantities of electricity, on metallic wires of the same or different diameters, is as the square of the passing quantity of electricity directly, and as the square of the diameter of the wire inversely. Thus the heat developed by a passing shock is four times as great when the quantity of electricity is doubled, and only one-fourth as great when the diameter of the wire is doubled. A metallic rod, therefore, of twice the diameter, conducts twice the quantity of electricity with the same development of heat. Taking the heat evolved as proportionate to the resistance, we may conclude, that the conducting power of a metallic body varies with the area exposed in cutting it transversly to its length; that is to say, with the area of its section, since this is proportionate to its solid contents.

In estimating the resistance of a metallic body to the transmission of a shock of electricity, we have also to take into consideration the distance traversed. Now the resistance to electrical transmission through a metallic wire, has been found to increase with the length of the wire; that is to say, the resistance to the transmission of the charge was twice as great when the length of the conductor was doubled; a law observable from one hundred up to one thousand feet in length. A lightning conductor, therefore, should have its dimensions increased, when required to be of considerable extent.

The explanation of the above general laws seems to be this: supposing a given quantity of electricity to fall on a single metallic particle, and to experience a given resistance to its progress, then this resistance would be diminished by placing a second particle by the side of it, for the charge would be divided between the two.—If two other particles were added, we may conceive it to be still further reduced in proportion to the number of particles sharing in the conduction. Now, in the case of the section of a metallic wire, the diameter of which is double that of another, there are four times the number of particles,—hence the resistance with the same quantity of charge is reduced to one-fourth. In a similar way it may be shown, that by increasing the length of the conductor, we continually increase the number of particles to be passed through,—hence the resistance through twice the length will be twice as great, through three times the length three times as great, and so on. In the case of the quantity of electricity being increased, we have the resistance dependant not only on the increased charge, but also on its increased force. Thus a particle of metal conducting a double quantity of electricity is subject to a double force, since it is quite reasonable to conclude that the forces with which the opposite electrical powers tend to unite, would increase with the amount of disturbance.

Mechanical Effects of the Electrical Discharge.—But it is not only the heating effect of the discharge we have to consider; it is necessary to take also into the account its more mechanical effects,—indeed, it is the expansive ac-

tion which produces the great mass of damage by lightning so commonly observed. If a powerful shock of electricity be transmitted by a fine wire, the wire will very often appear crippled throughout its length, and will exhibit a series of zigzag creases. And if a similar shock be passed between two metallic balls in a confined portion of air, the air will be caused to expand with great violence, so as to frequently burst open the containing vessel. Light bodies such as wafers will become dispersed in all directions, when exposed to the expansive effect produced by the electric shock in passing through a short interval of air.

At the time the steeple of St. Bride's church, London, was struck by lightning, in June, 1764, it is stated by Mr. Delaval, F. R. S., who communicated a most interesting detail of the circumstances to the Royal Society, that "the lightning acted as an elastic fluid," and that "the effects are exactly similar to those which would have been produced by gunpowder, pent up in the same places and exploded. A stone weighing about seventy pounds was thrown to a distance of fifty yards; an iron bar half an inch in thickness, and about two feet in length, joining some of the masonry of the tower, was not only broken in two, but one part of it, according to Dr. Watson,* was actually bent to an angle of 45°."

The mechanical effects of lightning, seen in piercing solid bodies with holes, in splitting them in pieces, and in projecting their fragments (sometimes of enormous weight) to great distances, are so well known, and so generally admitted, that it will be needless to multiply instances in proof of it; but a circumstantial statement of some remarkable cases of this kind may throw light upon the manner in which the electric fluid acts.

In the autumn of 1798, lightning struck the house of Casselli, an Engineer, at Alexandria. It did no damage, but pierced the panes of glass in the windows with several small holes about the sixth of an inch in diameter. Small cracks in the glass diverged from these holes as centres.

In August, 1777, lightning struck the steeple of the parish church of St. Sepulchre at Cremona, broke the

* *Phil. Trans.* for 1764.

iron cross which surmounted the tower, and projected to a distance the weathercock, which revolved under the cross, and which was made of copper, tinned, and coated with oil-paint.

This weathercock was found to have been pierced with *eighteen holes*, nine of which were very prominent on one side, and the other nine on the other. As there was no appearance of more than one stroke of lightning, all these holes must be supposed to have been pierced at once. The position of the holes is such as would have been produced by blows imparted simultaneously in opposite directions on parts of the metal nearly contiguous, and the inclination of the beards or projecting edges of the holes on one side correspond exactly with those on the other, the directions of all the eighteen beards being parallel.

On the 3d of July, 1821, lightning struck a house at Geneva, and pierced the tin which covered a part of the roof with several holes, leaving evident marks of fusion. One piece of tin in particular, which covered the angle made by a chimney with the surface of the roof near it, was pierced with three nearly circular holes, about an inch and three quarters in diameter, and about five inches apart measured from centre to centre. The metal at the edges of these holes was bent, as it would have been by a force bursting through it in one direction or the other. The edges of the two holes were bent on contrary sides.

On the night between the 14th and 15th of April, 1718, the church of Gouesnon, near Brest, was struck by lightning with such force that it shook as if by an earthquake. The stones of the walls were projected in all directions to a distance of from fifty to sixty yards.

The lightning which formerly struck the chateau of Clermont, in Beauvoisis, made a hole twenty-six inches wide and the same depth in the wall; the date of the building of which was so far back as the time of Cæsar, and which was so hard that a pickaxe could with difficulty make any impression upon it.

On the night between the 21st and 22d of June, 1723, lightning struck a tree in the forest of Nemours. The trunk was split into two fragments, one seventeen, and the other twenty-two feet long. These fragments, so heavy that one of them would require the combined

strength of four men, and the other that of eight men, to lift it, were, nevertheless, projected by the lightning to the distance of about seventeen yards.

In January, 1762, lightning struck the church of Breag, in Cornwall, the southwest pinnacle of the tower of which it destroyed. A stone, weighing one hundred and seventy pounds, was projected from the roof of the church to a distance of sixty yards in the direction of the south. Another fragment of stone was projected to the north to a distance of 400 yards. A third was projected to the southwest.

About the middle of the last century, a rock of micaceous schist, measuring 105 feet long, 10 feet wide, and about four feet thick, was struck by lightning at Funzie, in Scotland, and was broken into three principal fragments, not counting smaller pieces. One of these fragments twenty-six feet long, ten feet wide, and four feet thick, had been merely inverted in its position. Another, 28 feet long, 7 feet wide, and five feet thick, was projected over the hill to a distance of fifty yards. The remaining piece, forty feet long, was projected in the same direction, with still greater force, and fell into the sea.

On the 6th of August, 1809, at Swinton, about five miles from Manchester, lightning struck the house of Mr. Chadwick, at 2 P. M. A sulphurous vapor immediately filled the house. The external wall of a building erected against the house as a coal-shed, was torn from its foundations, and raised in a mass. It was transported, maintaining its vertical position, to some distance from its original place; one of its ends was transported nine and the other four feet. This wall thus raised and transported, was composed of seven thousand bricks, which, independent of the mortar by which they were cemented together, would have weighed about twenty-six tons. This wall was eleven feet high and three feet thick, and its foundation was about a foot below the level of the ground. Above this coal-shed was a cistern, which, at the time of the phenomenon, contained a quantity of water, and the shed contained about a ton of coals.*

The first Presbyterian church in the city of Syracuse, New York, was struck by lightning. The church had

* Dr. Lardner's Lect. on Science and Art, vol. ii. p. 69.

lightnining rods attached, but these were improperly fastened to the walls by sharp pointed staples in full connection with the main rod. The lightning struck the spire, ran down part way, when the charge divided, following a staple fastening into the building, as far as the pointed staple extended. This staple terminating in the dry, hard masonry which must have been nearly a non-conductor, the charge blasted out a portion of the steeple,—throwing the broken fragments several hundred feet much after the manner of a too shallow blast in a rock. And thus the beautiful structure was saved the dire destruction which must have happened had the church been built of wood, a partial conductor when wet, and fastened with iron bolts, &c. These facts were related to the writer a few months since by several intelligent and respectable citizens of that city who were eye-witnesses to the accident, each of whom saved peices that were thus blasted out of the steeple. The mechanical force here manifested was precisely like that of gunpowder.

In the shock of lightning which fell on His Majesty's ship *Elephant*, of seventy four guns, at Portsmouth, in November, 1790, all the iron hoops and wooldings on the mainmast were burst open and broken in peices, and some of the parts scattered to great distances. Some of these hoops were half an inch thick, and five inches wide; the mast, although of immense size, being about three feet in diameter, and upwards of 110 feet in length, was entirely shook and shivered throughout. Now, in these cases we do not find any melting of the iron work: we observe only the effects of a terrific mechanical power.

Another remarkably interesting instance of this effect was observed in the case of His Majesty's ship *Desirée*, already alluded to, struck by lightning in Port Antonio, in Jamaica, in the autumn of 1803. Admiral Ross, who then commanded the ship, states, that one part of the main topmast was found on the following morning sticking in the mud on one side of the harbor, and another part in a timber-yard on the opposite side.

In the application of lightning conductors to buildings, therefore, we have to consider the effect likely to be produced on them by the mechanical action of the shock, by which they may be disjointed, twisted or rent asunder in various ways. Thus, the small conductor of linked brass

rod, at Charles church, Plymouth, struck by lightning in December, 1824, was literally torn in pieces and disjointed, and many of the links twisted into the shape of the letter S. A part of the small wire rope applied as a conductor to the Hotel des Invalides at Paris was broken into small pieces an inch or more in length, and scattered in all directions by the lightning which fell on that building in June, 1839. This conductor consisted of about twenty iron wires twisted together as a rope; the lead surrounding the lantern was torn up and scattered, but without any signs of fusion.

The intensity of electrical accumulation having been found to decrease in an inverse ratio of the square of the opposed surfaces, some electricians were led to imagine that extent of surface was the great desideratum in the application of a lightning conductor. This decrease of intensity, however, does not affect the conditions of conduction as regards the heating effect of the discharge, for, whether the quantity of electricity be accumulated on a large extent of surface, or on a small one, the heating effect, when the discharge does occur, is always the same. Whatever may be the distance at which the neutralization of the forces begins, they always unite with precisely the same degree of power, the quantity of electricity being the same.

This question, so frequently discussed, has not been fully appreciated in all its details, for although quantity of metal is an essential condition of a lightning rod, yet it is likewise desirable to place the metallic particles under as great an extent of surface as may be consistent with strength and durability, in order to keep down the intensity of the shock, and diminish the mechanical action. We may, in fact, for the moment, consider a conductor, while in the act of carrying off a charge of lightning, as an electrified body. Its electrical intensity, therefore, is very much less with a large surface, than with a small one: hence, by extent of surface, we diminish the activity of the passing charge, and tranquilize its mechanical effect on the conductor. Thus we find, in a variety of cases of damage by lightning, that the passing charge, in striking on large expanded sheets of metal, has become comparatively tranquil, and has been traced no further, whilst in striking on larger masses of metal, exposing but a small

surface, it has assumed an intensely active state. The flash of lightning which struck His Majesty's ship *Badger*, lying in the Medway, in August, 1822, vanished, after striking upon the copper lining of the galley, although just before, it had penetrated the mast, rent the copper, and melted the lead over the heads of two large bolts in the deck beams.

Although the general tendency of a discharge of lightning is always through the conductor, as being the line of least resistance, yet there may arise cases in which the electrical forces may be so circumstanced as to affect other lines of action near it, and in which damage may ensue. Thus, in the French frigate *Calypso*, fitted with a wire conductor of small surface, two men in the main chains, who were standing in a slightly interrupted conducting communication between the wire and the sea were struck senseless. A similar case is related by M. Arago, as having occurred in the French frigate *Junon*. In fact, wherever from any cause the resistance in the direction of the conductor is considerable, the chances of damage in some other direction become multiplied. Thus, a case occurred at Bayonne, in which the length of the conductor was unnecessarily increased—that is to say, instead of terminating at once in the ground, it was led off at the foot of the building on semi-insulating stakes of wood for some distance; the charge here divided upon other lines near the conductor, and caused damage.

Provided the quantity of metal be present, the form under which we place it is evidently of no consequence to its conducting power, since it would be absurd to suppose that a mass of metal, under any form, did not conduct electricity in all its particles,—indeed, we know that it does so, and that it is impossible to fuse by electricity *a portion only* of a homogeneous metallic plate of uniform thickness. Again, if the heating effect of a given quantity of electricity on a metallic wire be measured, and then the wire be rolled out into a flat surface, or otherwise drawn out and placed under the form of two or more smaller ones, still the same heat will be evolved when conducting the same charge; there is consequently no disadvantage in giving a lightning rod as much superficial capacity as possible as regards conducting power, whilst, on the contrary, the diminished intensity attendant on it is very ad-

vantageous: this effect of superficial conductors appears to depend on the removal of the electrical particles further out of the sphere of each other's influence. In a dense solid rod they may be supposed to be in a state of compression, in each other's way, as it were,—whereas by expanding the same quantity of metal into a larger surface, we immediately free them from this condition, and allow them greater space.

In order, therefore, to resist the heating effect, we require quantity of metal; to restrain the electrical intensity, and to diminish the mechanical force, we require extent of surface. The distinction is nice but it is a very important one."

Important questions naturally spring from these facts. One of the first is: "What quantity of metal is requisite for a lightning rod?" and the following observations are to the point:

"Perhaps one of the most terrific discharges of lightning ever experienced, was that in the case of the New-York packet, struck by lightning in the Gulf Stream, in April 1827; already noticed. In this case the discharge fell on a pointed iron rod four feet long, and half an inch in diameter; some few inches only of the rod near its point were melted; the linked iron chain which descended from this rod to the water, about a quarter of an inch in diameter, was knocked in pieces by the expansive force of the shock, and some of the links fused. The flash of lightning not only melted some of the links, but "caused them to burn like a taper." "The melted iron fell in glowing drops upon the deck, which was instantly set on fire wherever the burning matter fell." "Such was the violence of the shock, that the ship recoiled, or, in sea phrase, lurched so strongly as to throw down the people on deck." The results of a great natural experiment are here presented to us, and we see that an iron rod of half an inch in diameter, effectually resisted a flash which fused and destroyed a chain of about one half of its dimensions.

The flash of lightning which struck Her Majesty's ship *Rodney*, in December 1838, was so powerful that it quite dispersed the topgallant-mast in small chips, covering the sea with splinters, set fire to the main-topsail, tore a piece

ten feet long out of the topmast, burst thirteen hoops on the mainmast, each five inches wide and half an inch thick, and traversed the mast with destructive effect for fifty-thee feet. Now this charge passed without fusion on the copper funnel, sixteen inches long, ten inchs in diameter, and less than a quarter of an inch thick, belonging to the topgallant rigging.

In September 1833, two successive discharges of lightning fell on His Majesty's ship *Hyacinth*, in the Indian Ocean, and descended by the fore and mainmasts to the sea. The topgallant and topmasts were literally shaken into a bundle of laths, so that they could scarcely be supported; the topmasts thus shivered were from eleven to twelve ieches in diameter and about forty feet in length. This destructive shock was safely conducted away from the termination of the topmasts by a chain sheet fifty feet long, made of iron rod half an inch in diameter, and finally through the ship by a copper pipe, three inches in diameter, one eleventh of an inch thick, and ten feet long; before reaching these bodies, which were direct conductors to the sea, the discharge ravaged and destroyed the masts through a distance of nearly eighty feet; the chain did not show any marks of fusion.

In the summer of the year 1760, a heavy discharge fell on a conductor fixed on a house in Philadelphia. This conductor consisted of an iron rod, half an inch in diameter; it extended nine feet above the chimneys, and terminated in an iron stake in the ground. The part above the chimneys was tipped with brass rod, about a quarter of an inch in diameter, and ten inches long. Mr. West, the owner of the house, judging from the crash that the conductor had been struck, had it examined. About three inches of the brass rod were observed to have been fused, and some of the fused metal had sunk down about the remaining part of the rod, forming a rough, irregular cap about it. The house was not damaged, nor were any further effects produced on the conductor.

On the 26th of January, 1838, a flash of lightning fell on Her Majesty's ship *Dublin*, of fifty guns, at Rio de Janeiro. It was carried off by a conductor of long copper links, each link being about a quarter of an inch in diameter, and ten inches long. The conductor was "reg-

ularly melted" in several parts; these fell on the deck; other parts, which remained attached to the line supporting the chain along the rigging of the ship, appeared as if they had been exposed to a very fierce heat. This line was not hurt, nor was any damage done to the rigging.

In June, 1772, a discharge of lightning fell on the Vicarage house at Steeple Ashton, in Wiltshire. The iron bell-wires in both the parlors and in the hall were entirely dispersed, *except in their twisted or double portions.*

In June, 1828, a heavy flash of lightning fell on the spire of Kingsbridge church, in Devonshire, and was received on an iron spindle, seven feet long, and one inch in diameter, without producing the least effect on it, or damaging the stones around it; but on leaving the spindle, however, it shattered the tower, and did considerable damage.

In the case of Charles church at Plymouth, although the wire was knocked in pieces at the points of junction, and considerably bent, it was not materially damaged by fusion.

In reviewing, in this way, a great variety of cases in which buildings and ships have been exposed to appalling storms of lightning in various parts of the world, and throughout a century of years, we are enabled to appreciate the power of metallic bodies to carry off lightning with safety. Now we do not find in any of these cases that a conducting rod, or other mass of metal, equal in substance or conducting power to a rod of copper half an inch in diameter and six inches long, has ever been fairly melted. On the contrary, heavy discharges have traversed rods of less dimensions with safety.

Besides, the conducting power of lightning rods has been much increased of late by improvements in their construction. 1st. In multiplying the number of their points. 2d. In perfecting their continuity. 3d. In the substitution of square rods for round ones. The reasons of these facts will appear hereafter. We will here stop only to add, what Mr. Harris, F. R. S., in his work on thunderstorms, affirms, viz, 'when an acutely terminated lightning rod receives a charge of lightning, a very

considerable portion of the charge, if not the whole, runs off in an attenuated stream.'"

The facts upon the range of "the protecting power of the lightning rod" are of great value, and the want of knowledge of these facts has led to many errors, and to consequent disasters. The reader will be well instructed in reading the following observations:

"It is not easy to assign the limit of the protecting power of a conductor. The French philosophers consider it will afford protection over a circle equal to twice its radius; this, although possible in certain cases, is by no means a general truth. All the experience we have of the operation of conductors on discharges of lightning, tends to the conclusion, that they have no influence whatever in determining the course of such discharges, farther than arises out of the circumstance of their furnishing an easy line of conduction. That they do not always afford protection over any considerable distance, is clear from the following cases:—

Her Majesty's ship *Endymion*, commanded by Captain the Honorable F. Gray, was struck by lightning at Calcutta, in March, 1842. This frigate had a chain conductor on the mainmast, applied in the usual way, not very dissimilar to that recommended in the report of M. Gay Lussac to the Royal Academy of Sciences at Paris. The electrical explosion, instead of falling on the conductor, struck the foremast, shivered the top-gallant and topmast, and damaged the lower mast. Now, in this case, the mast struck was not above fifty feet distant from the mainmast, which was furnished with a conductor, and had a radius of one hundred and fifty feet.

Her Majesty's ship *Ætna* was struck by several heavy electrical discharges at Corfu, in January, 1830. These, for the most part, passed down a chain conductor attached to the mainmast. One of the discharges, however, struck the ship near the bow, and exploded about twelve feet above the forecastle, close to the foremast, knocking down all the people on deck, and doing other damage.

The Board-house, at Purfleet, was struck by lightning on the 13th of May, 1777, at a point upwards of forty

feet from the conductor with which the house was furnished. The damage, it is true, was small; a few stones fastened by iron cramps not connected with the conductor were thrown down. The Board-house was a lofty building, with a pointed roof, well leaded, and connected by lead gutters and pipes with the earth, and with wells forty feet deep, for the purpose of conveying water forced up to a cistern in the roof. It was therefore only thought necessary to add an iron spike, about ten feet long, to the middle of the highest part of the roof. About one hundred and fifty yards from this building were five powder magazines, each one hundred and sixty feet long and fifty-two feet wide, having spiked conductors at each end projecting ten feet above the roofs, and connected with wells of water. It is quite apparent here, that so far as relates to the *influence* of a conductor over a given area, the experiment is conclusive, and the result shows that we cannot always calculate on the radius of protection; thus confirming the deductions already arrived at.

The Poor-house at Heckingham was struck by lightning on the 17th of June, 1781, which damaged one of the extreme corners of the building, situated seventy feet from the pointed conductors with which the house was furnished. Little or no damage, however, was sustained. The house consisted of a central range of buildings and two flanks; in general form, approaching that of the letter H. There were eight chimneys; each had a pointed conductor. The flash appears to have divided in this case before reaching the ground. One portion struck on one of the conductors, and was carried off; a second struck the extreme point of the building, and set it on fire; a third fell on the earth immediately in front of it.

The house at Tenterton, already referred to, had two stacks of chimneys at each end To one of these stacks was fixed a conductor of iron rod, projecting five feet above the chimney. Now, the discharge fell on one of the chimneys fifty feet distant, at the opposite end of the building; being the chimney diagonally opposite to that on which the conductor was placed, and on passing to the earth, it did considerable damage. The whole of this shock was finally concentrated on an iron bar three-quarters of an inch square, and produced no effect on it as already noticed. The house was about thirty feet wide."

In section third, the following points are investigated with ability, and the conclusions are reached with clearness: 1st. "Whether lightning rods and other metallic conductors have effectually guarded buildings, &c., against damage by lightning." 2d. "Whether lightning rods attract lightning." 3d. "Whether in certain cases pointed conductors actually prevent explosions of lightning from falling on buildings." Upon all these queries the facts gathered in this treatise are full of interest, and bear directly on the question discussed. The remaining points of interest in these investigations are: "Phenomena observed when a dense explosion of electricity falls on a lightning rod;" "Harmless character of the luminous appearances observed on lightning rods;" "Division of the charge;" "Instances in which buildings provided with pointed conductors are said to have been damaged by lightning;" "Precautions when exposed to thunderstorms;" and "Construction of lightning rods applied to buildings." There is a large amount of valuable, instructing and interesting matter under these various heads, which we should be pleased to quote, but our waning space admonishes us to restrict our extracts to the important point of Practical deductions from the facts and principles. We begin with Mr. Morgan's very perfect plan of securing buildings against damage by lightning.

"The plan which Mr. Morgan proposes is, that, whilst a house is being built, 'the foundation of each partition wall should be laid on a strip of lead, or a strip should be fastened to the sides of these partion walls. The strips should be two inches wide, and at least a quarter of an inch thick, and closely connected with each other. A perpendicular strip on each side of the house, should rise from the conductors to the surface of the ground; whence a strip should be continued round the house, and carefully connected with water-pipes, &c. The strips on the sides of the house should then be continued to the

roof, which ought to be guarded in the same manner as the foundation. The top should be surrounded by a strip, which should be connected with every edge and prominence, and continued to the summit of each separate chimney.' It is particularly necessary to guard the chimneys; for Mr. Morgan mentions a case in which a house that had been guarded in most respects, according to the preceding directions, except that the chimneys were unprotected, was struck with lightning, which entered by one of the chimneys; here it spent its fury; but the chimneys falling on the roof di considerable damage.

All electricians agree 'that security is rendered more perfect by having every piece of metal on the roof in metallic connection with the conductor, and continuous strips of lead built into every wall and connected to one another by horizontal strips communicating also with the conductor.'

The only objection to this method of protection is the expense. But even this is mainly obviated, by having the conductors so constructed as to run the whole length of the building on the ridge, with branches to the chimneys, and duly elevated above them; and, in case of small buildings, continuing the main rod on the ridge over the roof, down the opposite diagonal corners of the building to the ground; and if the building is large, by having rods extend from the main rod on the ridge over each end of the roof, down the four corners; and having all the conductors united to one another by a perfect metallic union."

The principles that are reliable in an effort to secure any object to be protected from damage by lightning are thus announced in Mr. Lyon's treatise:

"1. The conductor should be made of good conducting substance.

2. It should have great electrical capacities; a square rod requires less metal than a round rod.

3. It should be perfectly continuous, *i. e.*, it should have no breaks in the connection; no links or hooks, but a perfect metallic union in every part. If a person should think that a ring of zinc near these connections, improves the appearance of the rod, there can be no objection, though there is no other possible advantage.

4. It should be insulated from the building to be protected, except from such masses of metal as are likely to offer other lines of discharge.

5. It should have numerous lateral points:—one in six or seven feet will answer. The more numerous these points are, the greater the conducting power of the rod. Besides, these lateral points provide for an oblique discharge, each being as good a receiving point as the higher point at the chimney or other prominences. They also guard against a lateral explosion, or a division of the charge, which is liable to happen in case the rod is overcharged, especially if it be fastened to the house with pointed staples; and in case of an upward stroke, the electric fluid being discharged at so many different points, no harm can possibly occur.

6. Its upper extremity should project freely into the air; should be pointed; and may be triangular, somewhat similar to a bayonet; or it may have several branches. The only scientific advantage in having a branching head or point for the superior termination, is this: all the points are not likely to become blunt at the same time. Some have supposed that the point should be *magnetized*; and little needles, called "*magnets*," have sometimes been added. But is is difficult to see the practicability of this recent discovery; for most are aware, that magnetized iron or steel soon looses its magnetic influence. But, *is* there any truth or science in this application of magnetism? If there *is*, we confess, that we have not been able to discover it in any experiment in the laboratory; neither can we learn that the subject has even been *mentioned* by any writer *whatever*, on the subject of electricity.

7. The upper termination should be plated with silver or gold, to prevent corrosion.

8. Every branch rod running to chimneys, and other prominences, should have a perfect metallic union with the main rod.

9. In cases where metallic vane-spindles, or other points exist, the conductor may commence from these, and should be applied immediately to the part to be protected, and not at a distance from it; and should be so applied, that a discharge of lightning falling on the general mass, could not possibly find its way to the ground through the building by any circuit of which the conductor did not form

a part; that is to say, the conductor should be so carried over the several parts of the building, that the discharge could not fall upon it without being transmitted safely by the conductor. Hence, the rod should run along the whole length of the ridge, and down to the ground, at least on two sides of the building. If the building is large, it should run down on each corner.

10. Every conductor running to the ground, should terminate sufficiently beneath the surface, to insure moisture in the dryest part of the season. If circumstances permit, it should connect with a spring of water, a drain, or some other conducting channel."

We have thus endeavored to point out the principal elements of protection against the ravages of lightning, and medical men should endeavor not only to be able to advise correctly when consulted on this subject, but should be prepared to interfere when they see palpable blunders committed in an effort to disarm electricity of its terrible power. It must strike the reader that among the sources of success, there are none more important than a well made rod, securely united at the joints, perfectly insulated, and properly pointed. We have never seen any rod that so perfectly fulfilled these conditions as that made by Spratt. That conductor has no imperfections, and if it is put up according to the laws expounded in Lyon's treatise and quoted in this review, the protection may be regarded as very perfect.

We cordially thank Mr. Lyon for his seasonable treatise.

SELECTIONS

FROM

FOREIGN AND HOME MEDICAL JOURNALS.

Scarlet Fever. By M. F Colby, M. D.—Having just perused, in your Journal of the 16th of February, Dr. Wood's treatment of scarlet fever, I have taken the liberty of sending you my own views and practice in this disease. We country physicians not being so much guarded as others by rules of professional etiquette, are perhaps too apt to adopt independent rules of practice and to adhere to them whether right or wrong. Those who look with contempt on the practice and experience of country physicians, do so without reflection. Our opportunities for observation are far greater than such persons anticipate; and not being able to visit our patients so often, we are led to anticipate more closely changes which may occur in our absence. We cannot at all times call in a fellow practitioner, to return us a compliment which we may have made him, by strengthening our position with the friends by the declaration that all is right; but we have to make good our position and abide its consequences—and I can assure you that we are very strictly watched. You see, therefore, that we are often thrown on our own resources, in cases where a city physician could readily call in some one to divide his responsibility.

I have had formerly much experience in scarlet fever. In the year 1832 I had nearly 800 cases, which assumed all shades and all degrees of malignity, from the mildness of the flea-bite to the virulence of the plague. What puzzled me much, was the great diversity in the charac-

ter of the disease, and the great discrepancy among authors as to its nature and treatment. I found it breaking out here and there without exposure. In some cases several were sick in a family, and in others one or two; and besides, in some cases the rash would appear without the sore throat, in others the affection of the throat without the rash, in others both combined. In some cases in the same family it would manifest itself in enlargement of the parotid gland; but this usually occurred in infants at the breast. In others, the patient would be found in a state of collapse, with mottled, checked or purple appearance beneath or in the skin, without eruption or elevation of the surface. Now, thought I, can this be a contagious disease, exhibiting itself in such a diversity of forms? If so, why does it appear here and there without exposure? And besides, contagious diseases have something specific in their progress; but in this, all appeared chaotic, not only in its protean character, but also in the treatment as laid down by different authors. Now can order be brought out of confusion? With such reflections I carefully observed the different forms of the disease. At the same time I examined the throats of hundreds within its endemic influence, but who were not sick. In all these cases I found congestion and engorgement of the papillæ of the tongue, with elongation of the soft palate, and some degree of purple appearance about the fauces. The cases of collapse resembled the appearances which I had observed in those exposed to fixed air from burning charcoal, and I had also observed in the latter an eruption somewhat analogous to that of scarlatina.

From these facts I came to the conclusion that the cause producing scarlatina was atmospheric; that the first abnormal change was congestion or passive engorgement, or apoplexy of the mucous membrane, extending from the mouth through all its ramifications in the stomach and intestines, the bronchiæ and air cells of the lungs, as well as in the nostrils and Eustachian tubes to the internal ear. Now what is the first and immediate effect of this congested state of the mucous membrane? An impaired function of the lungs; the blood is imperfectly decarbonized, and so great is the suspension of the function of aeration, that in severe cases the patient falls into a state of collapse, and dies as he would from the respiration of fixed

air, without re-action—the dark-mottled appearance of the skin, in these cases, furnishing the only characteristic symptom as pointing out the nature of the disease. In slighter cases this first stage is not observed, and there appears no indication of illness till the eruption appears in the skin. In other more severe cases, the soft palate and fauces become dark and ulcerated, or the retained carbon and perhaps some other noxious principle which induced the disease is spent by irritation of the parotid glands. This engorgement of the capillary vessels, which constitutes essentially the disease (all the other morbid states being the result), produces as stated an impaired state of the function of respiration, or suspension of it. In the latter case the patient falls into a state of collapse, and dies; but when the function is impaired, the carbon is retained and its deleterious effect is manifested by the peculiar eruption which gives the name to this disease. In the congestive stage the vessels of the throat are so over-distended as to lose their vitality, and the parts fall into ulceration or gangrene. If this congestion be removed before the contractile power of the vessels is lost, they fall into a state of collapse; but the function of the mucous surface is not immediately restored—the membrane assuming a shining and glazed appearance. This state of the tongue is without doubt an index to that of the lining membrane of the stomach. By this condition of its mucous coat its function is impaired, and it is from this cause that so many fatal cases of relapse occur from improper food, even after slight cases of scarlatina. I believe that in the mildest cases of this disease, this red and glazed appearance of the tongue is ever an attendant, so much so that an experienced observer can tell by this the character of the disease.

Having taken this view of cause and effect, as constituting this disease in all its proteau characteristics, I was led to the adoption of the following practice, and twenty-five years' experience has confirmed my opinion of its correctness. In the early stage while there is still congestion in the mucous membrane, I give an infusion of Cayenne pepper, repeating every ten or fifteen minutes till some effect is observed in the lessening of the purple appearance in the throat—giving it, perhaps, six or eight times. I then give ipecac. to produce free vomiting; and in case

the throat is dark and somewhat ulcerated, I add sulphate of copper. In this case the pepper excites the vessels to action, and the concussion produced by the emetic is generally sufficient to relieve the oppressed membrane, and to restore the function of the lungs. If, however, there remain any purple appearance about the fauces and soft palate, I continue the pepper every hour till it is removed. I then treat the disease as mildly inflammatory. As this disease is one of extreme irritability, and the mucous coat of the stomach is in a glazed state, with its function impaired, I give but very little medicine. I usually give mucilages, and occasionally a little warm sage tea, and put the patient under the influence of belladonna. I use the German solution of the extract (three grains to the ounce). This I continue during the whole progress of the disease; and I am confident that no physician who has not given it a thorough trial is aware of its good effect. It lessens the violence of the disease, keeps out the eruption, and effectually equalizes the circulation in the capillary vessels. Since 1832 I have given it as a preventive, with invariable success whenever the directions were complied with. In fact, as I view the disease, no one can take it if brought under the influence of belladonna, for it effectually, by its action on the capillary vessels, prevents the congestion which primarily constitutes the disease. The tincture of colchicum and that of the Phytolacca Decandra, would unquestionably exert the same preventive powers. The relapse in scarlatina depends, no doubt, on the retention of too much carbon in the blood, with perhaps some of the deleterious miasm which first induced the disease, and as free action of the skin tends to eliminate this, Dr. Wood's practice would probably prove beneficial. If exposure to cold be avoided, and a careful regimen prescribed for a few days, there will be no danger of relapse. I think no solid food should be taken until the tongue has lost its glazed appearance. Cayenne pepper should never be used after the congestion in the mucous membrane is removed. I must, however, say, that, as far as I have observed the practice of others in this disease, the indiscriminate use of pepper, as practised by Thomsonians, has been far more successful than that of some physicians who treat it as inflammatory from its commencement (without reference to that passive state of engorgement

which ushers in the disease), with their calomel, antimony, and cathartics. The one unnecessarily keeps up irritation, but his practice has removed the first morbid condition; while the other permits the congestion to continue, until fatal results ensue, which are often accelerated by calomel and other irritating medicines.

The question may be asked—Do you consider this disease contagious? I answer—Certainly not. In 1832 it prevailed for 50 miles around me, ever occurring in isolated families. Why, then, do those who are exposed take it? They do not take it from the sick, but from the same endemic influence, and you will find its effects in examining all in the same locality. But why do several in the same family have it, while others escape? Because there is similarity of constitution, and the same susceptibility to receive morbid impressions. Such members would have had it if kept a mile apart in the same locality. Such cases I have known. Contagious diseases cannot be prevented by medicine, but belladonna will prevent the access of the disease (if the extract is good); and besides, contagious diseases do not expose to relapses, while exposure to cold or over-eating disposes to it in this, during any period within two weeks.—*Boston Med. and Surg. Journal.*

Wounds of the Heart. From Lectures by G. J. Guthrie, Esq., F. R. S.—Wounds of the Heart are for the most part immediately fatal. Many persons have, however, been known to live for hours, nay days, and even weeks, with wounds which could scarcely be otherwise than destructive; and several cases are recorded in which the cicatrices discovered after death, in persons known to have been wounded in the vicinity of the heart, have shown that even severe wounds of that most important organ are not necessarily fatal. As our knowledge of the nature of the injury inflicted can never be distinct, it follows that every wound should be considered as curable until it is unfortunately proved to the contrary.

Auscultation and *percussion*, and principally auscultation of the whole præcordial region, have afforded means of judging of injuries of the heart which were not form

erly known. A vertical line, coinciding with the left margin of the sternum, has about one third of the heart, consisting of the upper portion of the right ventricle, and the whole of the left, on the left. The apex of the heart beats between the cartilages of the fifth and six left ribs, at a point about two inches below the nipple, and an inch on its external side; or, if one leg of a compass be fixed at a point midway between the junction of the cartilage of the fifth rib on the left side, with the rib and the sternum, and a circle of two inches in diameter be drawn around, it will define as nearly as possible the space of the præcordial region occupied by the heart—whilst uncovered except by the pericardium and some loose cellular texture. In the rest of the præcordial region it is covered and seperated from the walls of the chest by the intervening lung.

If the chest of the dead subject be transfixed with long needles, it will be found that the centre of the first bone of the sternum corresponds with the lower edge of the left subclavian vein, and to the arch of the aorta crossing the trachea; the centre of the second bone to the upper edge of the appendix of the right ventricle; and the centre of the third bone corresponds to the right side of the right auricle; the right ventricle being lower down. A needle penetrating the chest at the costal extremity of the fifth rib, close to the upper edge of its cartilage, will touch the septum of the ventricle. The apex of the heart is an inch and a half below this, and inclined to the left side.

The semilunar valves of the pulmonary artery correspond to a spot a little below the centre of the third bone of the sternum. The aortic valves are a few lines below and behind the pulmonary. The mitral valves are a little lower, and still more deeply seated. The pulmonary artery, after touching the sternum inclines to the left, and is found close to the sternum, between the second and third ribs. The aorta ascends to the first bone, and crosses it to form the arch.

One third of the heart, consisting of the upper part of the right ventricle and of the whole of the right auricle, is beneath the sternum; the remainder of the right, with the left ventricle and auricle, are to the left side of that bone.

On applying the ear to the præcordal region, the patient being in the erect position, two sounds are distinguishable in a healthy heart—one duller and more prolonged, the other clearer and shorter; between these there is scarcely an appreciable interval. The period of repose is sufficiently marked before the first or duller sound returns. Of the time thus occupied, one half is filled up by the first or dull sound; one quarter by the second or sharp sound, one quarter by the pause or period of repose.

Twenty-nine theories have been proposed, each accounting for the sound of the heart. The theory of Dr Billing appears to prevail at present, which supposes that the sounds thus heard "are caused by the valves, which, being membranous, each time they resist the reflux of the blood, are thrown into a state of sudden tension, which produces sound."

The impulse of the heart, so far as it can be felt by the touch, depends much on the position in which the body is placed. In the erect position it is heard between the fifth and sixth ribs. In the recumbent posture the impulse is almost imperceptible. It is, perhaps, more observable when the body is turned on the right side, but decidely more so when it is turned on the left. A clearer sound proceeds from a thin, and a duller sound from a thick heart; a sound of greater extent from a large heart, and a sound of less extent from a small one. A more forcible impulse is given by a thick heart, and one more feeble by a thin one; the impulse is conveyed to a longer distance from a small heart.

From a clearer sound we believe in the probability of an attenuated heart, but we argue its certainty from a clearer sound joined with a weaker impulse. A stronger impulse denotes the probability of an hypertrophied heart, but we argue its certainty from a stronger impulse with a diminished sound.

The terms endocardial and exocardial are used to designate the alterations which take place in the sounds of the heart under disease; endocardial when they occur within the heart, and exocardial when they take place upon its surface. The endocardial murmur of disease, or bellows sound, takes place of, and is substituted in certain cases for, the first or second, or even for both the healthy or normal sounds. The exocardial murmur of

disease is heard with the normal sounds, but confusing and overpowering, sometimes overwhelming them by its rubbing or crumpling noise. The natural sounds exist, although rendered imperceptible by the greater distinctness and nearer approach of the unnatural or unhealthy ones.

The heart apart from the pericardium never moves without a sound; the pericardium apart from the heart never gives out one. Under disease the heart gives out the natural sound, diminished, exaggerated, or modified, or, it may be, totally altered. The sounds given out by a diseased pericardium must always be new (there being no old ones), and are described as rubbing, or to-and fro sounds. The pleura when diseased, being a serous structure, like the inner membrane of the pericardium, gives out less marked, but somewhat similar sounds (the "*frottement*" of the French), in particular stages of the disease.

The alterations in the ordinary sounds constituting the endocardial murmurs of the heart heard under disease, depend principally on the altered state of the endocardium, or membrane lining its cavities; the sounds given off, and called exocardial, on an altered state of the serous membrane of the pericardium, are reflected over the outer surface of the heart. The endocardial or bellows sound, when it accompanies the normal sounds of the heart, may result from any kind of derangement affecting the internal membrane of that organ, particularly rheumatic inflammation, or from any force which may compress its cavities; it may depend on the altered quality of the blood, from anæmia. It should be present after excessive hæmorrhages have greatly reduced the powers of the sufferer. When this murmur or sound occurs after injury in the vicinity of the heart, and is accompanied by fever, it indicates inflammation of the lining membrane, although no local pain, no palpitations, no irregular movements of the heart should be present.

When a murmur or sound is heard of a different kind, possessing the character of friction, or of surfaces moving backward and forward on each other, or to and fro—this sound is the sign of inflammation of the membrane covering the heart, as well as of that lining the fibrous external tissue of the pericardium. The signs of both exter-

nal and internal inflammation may be present at the same time, and they frequently are in cases of acute rheumatism.

When the heart is supposed to be wounded, even without much loss of blood, there is fainting, palpitation, irregular movement or total cessation of its action; coldness of the extremities; ghastliness of countenance, succeeded by great anxiety; a sense of anguish; an intermission or cessation of pulse, followed, if the patient should survive, by re-action, which renders it very frequent, and sometimes increases its impulse, whilst the anxiety is increased by pain, sometimes intolerable, referred to the part. These symptoms imply a serious injury, although they may not all be present, and many of them differ in intensity. If the patient should survive, the ordinary sounds of the heart will return, with more or less irregularity, accompanied after a few hours by the endocardial murmur, although something like it may perhaps be observed from the first period of injury. The friction, or attrition sound, indicating the presence of inflammation of the pericardium, may be absent, and will not be discernible, if a layer of blood is effused into the cavity of that membrane, whilst the natural sounds of the heart are rendered more indistinct as the heart is separated from the walls of the chest by the effusion, which distends the pericardium, and impedes the regular action of, but cannot compress the heart, as an empyema does the lung. If inflammation take place without an effusion of blood, the friction sound will be heard, and will usually continue even after some effusion of serum and of lymph have occured, as the quantity of serum is rarely sufficient to prevent the effused and attached portions of lymph from rolling against each other.

The presence of a larger quantity of fluid may be more distinctly known by percussion; if it can be borne in cases of injury, the degree and extent of the dulness being the measure of its existence and accumulation. It may extend over a part or the whole of the præcordial region, reaching as high as the second, or even the first rib, beneath the sternum, and even under the cartilage of the ribs of the right side.

That the heart when wounded is capable of recovery by the permanent closure of the wound, in a few rare in-

stances, is indisputable; and it would seem, from a consideration of the different cases which have been recorded, that such recovery takes place in consequence of there being little blood discharged through the wound, or into the cavity of the pericardium, or into that of the pleura.

The absence or the cessation of hæmorrhage, by the contraction of the wound, or the formation of a coagulum, is the first step towards a cure, and it was to one or other of these circumstances that most of those who survived the injury for several days or weeks owed their existence for the time, although they usually died from the effects of inflammation, more of the inner lining and outer covering, than of the substance of the heart itself.

If the wound be inflicted by a musket or pistol-ball, it cannot be closed, although pressure may be made upon it for a time, so as to suppress the external flow of blood. If this should succeed, it is more than probable that the hæmorrhage will continue internally, and that the patient may die after much suffering, principally from oppression, caused by the escape of blood into the cavity of the chest.

If the wound be a stab, the external opening may be accurately closed, and the escape of blood prevented; but as the pressure of the blood in the pericardium is unequal to restrain the action of the heart, blood forced out through the opening fills the cavity of the pleura, and causes suffocation, unless from some accidental circumstance the opening in the heart becomes obstructed and the bleeding ceases.

If all the circumstances be considered, there can be no doubt of the propriety of closing the wound in the first instance, if the flow of blood is excessive and appears likely to endanger life. It seems to be as little doubtful that the wound should be re-opened after a time, if the danger from suffocation be imminent. The relief obtained by the escape of a little blood may be efficacious, whilst it does not necessarily follow, although it is more than probable it will be so, that its place will be occupied by a further extravasation of blood, which will prove fatal. It is a choice of difficulties, and death from hæmorrhage is easier than death from suffocation.

In the case of the Duke de Berri, whose right ventricle was wounded, and who died from loss of blood, Steifen-

sand reprehends Dupuytren for having opened the external wound every two hours to prevent suffocation; but if death were actually impending from the filling of the cavity of the chest being about to cause suffocation, there was nothing to be done but to give relief at all hazards.

When the sufferer has recovered from the imminent danger attendant on the infliction of the injury, and the pericardium is believed to be so full of blood or of serum as to prevent in a great measure the movements of the heart, it has been proposed by the Baron Larrey to open the pericardium by the following operation—equally, as he thinks, applicable in an ordinary case of hydrops pericardii:—

"An oblique incision is to be made from over the edge of the ensiform cartilage, to the united extremities of the cartilages of the seventh and eighth ribs. The cellular tissue being divided with some fibres of the rectus and external oblique muscles, there remain only a portion of the peritonæum, called its false layer, above the pericardium, which can be seen after the division of all intervening cellular tissue, projecting between the first and second digitations of the diaphragm. Into this the bistoury is to be entered, with the precaution of doing it with the edge turned upward, and directed a little from right to left, to avoid the peritonæum. The smallest portion possible of the anterior border of the diaphragm is next to be divided, where it is attached to the inner part of the cartilage of the seventh rib. The internal mammary artery is to the outside. The patient should be placed perpendicularly, and supported on his bed, which inclines the anterior part and base of the pericardium to the fore part of the chest."

Skeilderup recommends this operation to be done by trepanning first the sternum, a little below the spot where the cartilage of the fifth rib is united to that bone, at which part the periosteum lining it offers considerable resistance, and should not be divided by the trephine. Below this there is a triangular space formed by the separation of the layers of the mediastinum free from cellular tissue, and tending a little more to the left than to the right. The apex of this triangle is opposite the fifth rib; its base touches the diaphragm. The bone having been removed, the patient is made to lean forward, when the projection of the pericardium will enable the operator to

feel that a quantity of fluid is within, and to open it with safety.

J. Dierking, a stout muscular man, of the third regiment of German Hussars, was wounded at the battle of Waterloo by a lance, which penetrated the chest between the fifth and sixth ribs, and was withdrawn. He fell from his horse, lost a good deal of blood by the mouth, and some by the wound, and was carried to Brussels without any particular attention being drawn to the injury. His strength not being restored, whilst he suffered from palpitations of the heart, and other uneasy sensations in the chest, he was sent to England to be invalided, and in November, 1815, to York Hospital, Chelsea, in consequence of an attack of pneumonia, of which he died in two days, without attention being particularly drawn to the cicatrix of the wound.

On examining the body I found that the lance, having injured the edge of the cartilage of the rib, passed through the inferior lobe of the left lung, the track being marked by a depressed narrow cicatrix. It then perforated the pericardium under the heart, and sliced a peice of the outer edge of the right ventricle, which being attached below turned over and hung down from the heart to the extent of two inches, when in the fresh state, the part of the ventricle from which it had been sliced being puckered and covered by a serous membrane like the heart itself. The lance then penetrated the central tendon of the diaphragm, making an oval opening, easily admitting the finger, the edges being smooth and well defined. It then entered the liver, on the surface of which there was a small irregular mark or cicatrix. The heart in front was attached to the pericardium by some strong bands, the result of adhesive inflammation, but the general appearance of the serous membrane showed that this had not been either great or extensive. The pericardium was not thickened.

If this man had lived long enough, he might have furnished an instance of hernia of the stomach or intestine into the pericardium.

That the heart when exposed is insensible, or nearly so, to the touch, was known to Galen and to Harvey. Galen is said to have removed a part of the sternum and pericardium, and to have laid his finger on the heart.

Harvey did the same on the son of Lord Montgomery, who was wounded in the chest. Prof. J. K. Jung not only introduced needles into the hearts of animals, but also galvanised them without disadvantage, although Admiral Villeneuve is supposed to have died suddenly from running a pin into his heart with a suicidal intention.

That a person may die from the shock of a blow on the heart, need not be doubted, and that they do die when little blood is lost, is admitted. History preserves the fact, that Latour d'Auvergne, who had obtained the honorable title of "Premier Grenadier de France, and Captain of the 46th demi-brigade," fell and died immediately after receiving a wound from a lance at Neustadt, in the month of July, of the sixth year of the Republic, which struck the left ventricle of the heart, near its apex, but did not penetrate its cavity. He was, however, 68 years of age.

In wounds of the heart, all extraneous matters should be removed, if possible, and all inflammatory symptoms should be subdued by general bleeding, by leeches, by calomel, antimony, opium, &c. The chest should be examined daily by auscultation. If the cavity of the pleura should fill with blood, it ought to be evacuated to give a chance for life, and if the pericardium should become permanently distended by fluid, it should be evacuated.

Lacerations and ruptures of the heart have frequently taken place from blows or other serious contusions.

Ollivier, who devoted much time to reading and collecting the observations made by different writers on the injuries of the heart, says, "that of 49 cases of spontaneous rupture of the heart, 34 were of the left ventricle, 8 only of the right, 2 of the left auricle, 3 of the right, and that in 2 cases both ventricles were torn in several places; and that these results were in an inverse proportion to those which occurred after blows or contusions; the right ventricle being ruptured in 8 out of 11 cases, the left ventricle 3 times; the auricles being also torn in 6 out of these 11 cases; the raptures not being confined to one spot, but taking place occasionally in several different parts, or even in the same ventricle." In 8 of the cases he had noticed, the heart was ruptured in several places. That a spontaneous rupture may be cured as well as a wound, seems likely from a case reported by Rostan, of a woman

who died after fourteen years' suffering with pain about the heart, and was found to have the ventricle ruptured. A cicatrix was observed to the left side of the recent rupture, half an inch in extent in every direction, and in which the new matter was evidently different from the natural structure of the heart.—*London Lancet.*

Pills of Sulphate of Quiniae.—Dr. Pereira directs the pills to be made with conserve of roses, and in the three formulæ given in Pharmacopée Universelle, crumb of bread, honey and conserve of roses, are directed. Dorvault directs in L'Officine, for disulphate, the extract of wormwood; for the acid sulphate, conserve of roses. The pills are not officinal in either of the British Pharmacopœias. In our own officinal directions, in the edition of 1850, gum arabic and honey are prescribed, while in that of 1840 gum arabic and syrup were the excipients.

The use of gum arabic and syrup was abandoned on account of the pills becoming insoluble by keeping. Gum arabic and honey used together are probably less objectionable. The omission of the gum entirely is perhaps an improvement, honey answering the purpose alone. As quinine is now more frequently prescribed in 2, 3 and 5 grain doses than in the one grain dose that used to be given, it is a desideratum to use an excipient which will produce the smallest possible increase of bulk at the same time that it gives a plastic mass.

The following formula is, I think, preferable to those in which gum arabic is employed, as well for the diminutive size as for the increased solubility of the pills:

Take of sulphate of quinia	12 grains.
Powdered tragacanth	1 grain.

Triturate the powders thoroughly together, and add sufficient water to form a plastic mass. Divide this into the required number of pills. Made in this way a three grain pill is not inconveniently large.

The use of simple water as an excipient is, I am told, common in domestic practice in the Southern States. The mass produced in this way possesses too little adhesiveness to render it satisfactory. Tannic acid has been used of late with a view to diminishing the intense bitter-

ness of the quinine, but has not found favor generally, as far as my observation has extended. How far the known insolubility of the tannate of quinia in water should operate against this combination is a question for the therapeutist. Conserve of roses, in addition to its bulk, may be objectionable in this as in some other cases, on the score of containing tannic acid, which it does when made from Rosa gallica.

The following formula I have used for several years with great satisfaction to myself and to those physicians who have prescribed it. It was first suggested by a southern *medical student.*

Take of sulphate of quinia, 20 grains.
Aromatic sulphuric acid, 15 drops.

Drop the acid into the sulphate of quinia on a tile or slab, and triturate with a spatula until it assumes a pillular consistence; then divide into the required number of pills. Made in this way a five grain pill is not inconveniently large.

Although the ingredients when mixed form a fluid, they soon thicken into a paste, and finally become quite solid, and so adhesive as to be readily divided and rolled into pills; care must be taken not to allow the mass to become too dry and brittle before dividing it, as it is liable to do if allowed to remain too long.

In this form a portion of the disulphate being converted into the soluble neutral sulphate, the preparation more nearly resembles the solutions in composition, and is believed to be more rapid and certain in its action.

When it is desired to incorporate other substances in powder with the quinine thus prepared, they should be added to the mass when it is just so soft that, upon their addition, it will immediately assume the proper consistence.

It is not, however, advisable to employ this process when any considerable quantity of other ingredients are prescribed with the quinine, unless a little syrup or honey is also added to prevent the too rapid hardening and consequent crumbling of the mass.— *American Journal of Pharmacy.*

Recent Researches of Prof. Agassiz.—Prof. Agassiz has recently made a rapid tour from Charleston, S. Carolina, through Alabama, Mississippi, and Louisiana, thence up the Mississippi to St. Louis, Chicago, and along by the Great Lakes to New York and Massachsetts. In a recent letter from him, addessed to J. D. Dana, dated Cambridge, June 9th, he mentions the following as some of the results of his tour.

"I have been successful in collecting specimens, especially fishes, of which I have brought home not less than *sixty* new species, mostly from the great southern and western rivers. Some of these are particularly interesting. I would mention foremost a new genus, which I shall call *Chologaster*, very similar in geneal appearance to the blind fish of the Mammoth Cave, though provided with eyes; it has like Amblyopsis *the anal aperture* far advanced *under the throat*, but is *entirely deprived of ventral fins*; a verystrange and unexpected combination of characters. I know but one species, Ch. cornutus, *Ag.* It is a small fish scarcely three inches long, living in ditches of the rice fields in South Carolina. I derive its specific name from the singular form of the snout, which has two hornlike projections above.

The family of Cyprinodonts has received the most numerous additions, and among them there are again new combinations of characters. Several years ago I noticed two species of a new genus which I would call *Heterandria*, from the great difference observed between the two sexes, the males having the venetral fins near the pectorals in about the same position as in the *Thoracic* fishes, while the females have those fins in the middle of the belly as in the *Abdominals*. Of this genus I have observved several new species They all live in dense schools in shallow waters. Another type, *Zygonectes*, presents no such sexual differences, and differs also in its habits. These fishes are constantly seen swimming on the top of water *in pairs*, whence their name. I have found half a dozen new species of this genus.

You may remember the remarkble genus Mollinesia described by Lesueur from specimens obtained from Lake Ponchartrain and from Florida. If you do not, pray look for the figures in the Journal of the Acad. of Nat. Sci., vol. 2, to appreciate the facts here mentioned. From

its structure and from the sexual differences observed among other Cyprinodonts, I have long entertained the opinion that this genus had been established upon the males of Pœcilla mutilineata also described by Lesueur (same Journal), and both admitted as distinct in the great natural history of fishes by Cuvier and Valenciennes. Having found both together in all the Gulf States, I have watched them carefully, and in Mobile as well as in New Orleans, I have seen them day after day in copulation during the months of April and May; so that their specific identity is now an established fact. I have caught hundreds of them and found all the Pœcilias to be females and all the Mollinesias males; and what is further very interesting, the females are viviparous. I have been able to trace their whole embryonic development in the body of the mother, in selecting specimens in different stages of gestation.

I do not remember whether I have already mentioned to you the existence in the United States of two families of fishes not before observed in our waters, one the *Myxinoids* with one species from Eastport in Maine, collected by W. Stimpson, the other the *Erythrinoids* of Valenciennes, or *Characini* without adipose fin of T. Müller of which a new genus occurs in the freshwaters of our northern and middle as well as western States, with half a dozen species, some of which have been unforunately described as Lucicus, Fundulus and Hydrargyra, with which genera they have no affinity, while other new ones have been discovered by Professor Baird and myself. I shall call this genus *Melanura*, from the singular black mark which all species show upon the tail. But I would tire you were I to go on with my ichthyological remarks, even if I should limit myself to enumerating new genera, for I have many more of these.

I will close this long letter with one observation upon Crustacea, which may have a more immediate interest for you, if you have not yet noticed the fact yourself. On my return from Florida two years ago I noticed among many specimens of Lupa dicantha, one in which the tail presented a triangular form intermediate between that of the male and that of the female. Unable to ascertain from a single specimen whether it was a mere variety or perhaps an improperly developed female, I awaited another opportunity for a fuller investigation, which the market of Charles-

ton, S. C. afforded largely during the latter part of February last, when I ascertained that that form was at times as common in the market as either the males or the females, and upon careful anatomical examination, I satisfied myself farther, that these specimens *are entirely deprived of internal sexual organs*, though slight indications of the openings of the sexual organs, entirely closed up by the calcareous matter, clearly indicated that they are imperfectly developed females, a kind of neuters among crabs, the great number of which leads to the supposition that they are not without function in the general economy of these animals. The tail is soldered to the carapace, the last joints only at which the alimentary canal terminates, being moveable. At the same time males and females were dissected, and showed the sexual organs in that fullness which precedes copulation. Looking afterwards for similar conditions in other species, I found in the collection of Professor L. Gibbes, specimens of L. pa cribraria, and of L. Sayi with the same conformation of their tail.

I cannot help returning to my fishes to say, that I have now *twenty species* of Lepidosteus from the United States in my collection, a good foundation upon which to base a revision of the fossil fishes. I could not say how many other new things I have collected, for I have not yet unpacked half my packages."—*Silliman's Jour.*

Remarkable case of Catalepsy. — Cornelius Vroomer, now aged about 37 years, a country laborer, and resident of Clarkson, Monroe Co., N. Y., of robust and vigorous frame, middle size, and 165 lbs. weight, about five years since (in June, 1848) made complaint of an obtuse pain in his head and epigastrium. This was sufficient to induce him to stop work, and ask medical advice. Dr. C., of Clarkson, saw him and prescribed little more than guaiacum and rhubarb, supposing the case one that would prove amenable to mild measures. The pain in the head and stomach continued for about two weeks, when consciousness, sensorial powers and volition became suspended. His respiration and circulation continued natural, while, without coma or spasm, muscular rigidity retained the body and limbs in the precise position they were when

the attack came on. No apparent change occurred from this condition for several days, and with the exception of waking periods of from one to sixteen hours taking place at intervals of from one week to four months, he has remained in the same condition to this date.

His conscious periods for the first few months occurred at intervals seldom exceeding two weeks—latterly seldom less than four, and sometimes not less than eight or ten weeks. His most lengthy period of unconsciousness has been four months; longest waking interval sixteen hours. In the great length of the periods of unconsciousness and motionless rigidity, the case is unique and particularly interesting. During the whole of these periods there has been the most complete anæsthesia. Powerful excitants have been applied to the surface at various times, such as blisters over the head and spine, and at one time, accidentally, sufficient heat to his limbs to vesicate extensively, without causing any manifestation of pain. The nervous system, it would appear, is but slightly influenced by all external impressions. None but the slightest motion of the limbs are induced by current of magnetic electricity, and under repeated application of cold water, including a plunge into Lake Ontario, he seemed to experience no impression, excepting at one time under a long-continued douche from a height, he moved himself from his position, but did not manifest a full return of sense.

The involuntary functions of his system are performed with much regularity, but with less force and activity than in health. Pulse and respiration regular but faint. Evacuation from his bowels occurs once in from six to nine days—passes his urine more frequently. These calls of nature are made manifest by a slight uneasiness of his body, and are kept in abeyance until necessary preparations are made. From this feature of the case, and the fact that when conscious he has once or twice evinced a knowledge of events which transpired during his cataleptic state, and that he possesses the power of balancing himself when placed erect upon his feet, it is apparent to the physiologist that everything but the spinal and ganglionic system is not asleep, that the senses are partly awake, and of a somnambulic character. I am informed by his attendants that there are times when his

system seems more completely under the influence of natural sleep than at others; this soporose state is indicated by snoring, and is not attended with the least relaxation of the muscular rigidity. Food is usually administered to him twice a-day, and only by prying the jaws asunder and forcibly introducing it into his mouth and fauces.

His conscious periods return suddenly and at irregular intervals. During these short waking periods he walks erect, shows no very manifest diminution of strength or natural mobility, greets his old acquaintances, converses freely, and supplies liberally the demands of his stomach. On one occasion, not many weeks since, it is said he walked to the beach of the Lake, a distance of half a mile, and treated himself and friends to a supply of such grocery luxuries as dried herring and crackers.

Nothing is known of his habits previous to the invasion of this disease, to which it can be referred. It is known that he "imbibed" only occasionally, and was a tobacco chewer, and it is worthy of note that the last-named habit is completely reformed, as he has not expressed a desire for the weed in four years. During the colder months the temperature of his surface is considerably diminished, and at time his extremities are quite cold. When the surrounding atmosphere is comfortably warm, the temperature varies but little, if any, from the healthy standard.

As seen at this time his cast of features is calm and complacent, and presents nothing particularly unpleasing, except the protrusion of his lips. The firm pressure of his jaws has forced the incisor teeth to a very acute angle in front, and almost forced them from their sockets. The projection of the teeth and the approximation of the maxillæ have forced his teeth to a prominence quite equal to his nose. The external surface of the upper lip lies in close proximity with the nostrils.

Tonic spasm, of not the greatest rigidity, exists throughout his muscular system. His head is brought downward and forward towards the sternum; the body is curved forward, the spine forming the arc of a circle, as in emprosthotonos. The forearms cross the abdomen; the wrist joints and those of the fingers are the most firmly fixed. The thighs and knees are but slightly flexed, and are firmly fixed at obtuse angles. When placed upon his feet or seated in a chair, he has the power of sustaining himself in

an erect posture as rigid as a statue, from which position he has to be removed mechanically by his attendants. At one time he was placed upright upon his feet, and suffered to remain in his motionless position for three days and nights.

It is unecessary to specify the various measures which have been adopted by a number of the sons of Æsculapius for his restoration. Their "science and skill," thus far, have seemed to avail nothing in this case. His friends say they have restored consciousness on one or two occasions by administering very copious draughts of whiskey. Nitrous oxide, long continued cold to the head and spine, and chloroform, are measures which have not been tested.—*N. Y. Democrat.*

Coffee-Leaves as a Substitute for Tea.—From the *Overland Singapore Free Press*, published Jan. 3, 1853, we extract the following letter signed "*An old Sumatran*," upon the use of coffee leaves for the preparation of a beverage in the island of Sumatra. We briefly alluded in the *Pharmaceutical Journal* for June, 1852 (Vol. xi., p. 578,) to a project for employing coffe-leaves in this country as a substitute for tea.

"In the *Singapore Free Press*, of the 17th of September last, are extracts from the *Colombo Observer*, by which it appears a patent has been taken out by Dr. Gardner (known to us by his travels in South America*) for preparing the coffe-leaf in a manner to afford a beverage like tea, that is, by infusion, 'forming an agreeable, refreshing, and nutritive article of diet.'

"It may be interesting to Dr. Gardner, his friends, and the public in general to learn, that an infusion of the coffee-leaf is an article of universal consumption amongst the natives of this part of Sumatra; wherever coffee is

*It is Dr. John Gardner of London who exhibited prepared coffee-leaves at the Great exhibition of 1851. Mr. George Gardner, late superintendent of the Botanical Garden at Peradenia, Ceylon, and author of *Travels in the Interior of Brazil*, died in Ceylon in March, 1849. —Ed. *Pharm. Journ.*

grown the leaf has become one of the very few necessaries of life which the natives regard as indispensable.

"The coffee plant in a congenial soil and climate exhibits great luxuriance in its foliage, throwing out abundance of suckers and lateral stems especially when from any cause the main stem is thrown out of the perpendicular, to which it is very liable from its great superincumbent weight compared with the hold of its roots in the ground. The native planters, availing themselves of this propensity, often give the plant a considerable inclination not only to increase the foliage, but to obtain new fruit-bearing stems when the old ones become unproductive. It is also found desirable to limit the height of the plant by lopping off the top, to increase the produce and facilitate collecting it, and fresh sprouts in abundance are the certain consequence. These are so many causes of the development of a vegetation which becomes injurious to the quantity of the fruit or berry unless removed; and where this superabundant foliage can be converted into an article of consumption, as hitherto the case in Sumatra, the culture must become the more profitable, and it is clearly the interest of the planters of Ceylon to respond to the call of Dr. Gardner, and by supplying the leaf on reasonable terms, to assist in creating a demand for an article they have in abundance, and which for the want of that demand is of no value to them. It ought to be mentioned also, that the leaves which become ripe and yellow on the tree and fall off in the course of nature, contain the largest portion of extract and make the richest infusion, and I have no doubt, should the coffee-leaf ever come into general use, the ripe leaf will be collected with as much care as the ripe fruit.

"The mode of preparation by the natives is thus:—The ends of the branches and suckers with the leaves on, are taken from the tree and broken into lengths of from twelve to eighteen inches. These are arranged in the split of a stick or small bamboo, side by side, forming a truss in such a manner, that the leaves all appear on one side and the stalks on the other, the object of which is to secure equal roasting, the stalks being thus exposed to the fire together and the leaves together. The slit being tied up in two or three places, and a part of the stick or bamboo left as a handle, the truss is held over a fire with-

out smoke, and kept moving about so as to roast the whole equally without burning, on the success of which operation the quality and flavor of the article much depends. When successfully roasted the raw vegetable taste is entirely dissipated, which is not the case if insufficiently done. When singed or overdone, the extract is destroyed and the aroma lost. When the fire is smoky, the flavor varies with the nature of the smoke. The stalks are roasted equally with the leaves, and are said to add fully as much to the strength of the infusion. By roasting, the whole becomes brittle, and is reduced to a coarse powder by rubbing between the hands. In this state it is ready for use, and the general mode of preparing the beverage is by infusion, as in the case of common tea.

"If the testimony of one who has been long personally accustomed to the use of an infusion of the coffee-leaf thus prepared, can be of any avail in recommending the article to public notice, I freely offer mine in support of all that which Dr. Gardner's patent claims for it, viz., 'as forming an agreeable, refreshing, and nutritive article of diet.' While I find the use of infusion of the *berry* for a few days invariably to produce on me, as on many others, the effects of nervousness and bilious obstruction, I drink a strong infusion of the *leaf* daily with evident benefit to my health and strength. As a restorative on exhaustion from the severities of labor or of the weather, from heat to cold, or long exposure to rain, I know nothing superior to it. It has also the advantage of being a powerful disinfectant, so far as neutralizing fœtidity goes, and a solvent of the viscid fluids which obstruct the circulation, often to the extent of becoming laxative if taken in extra quantity. Of its nutritive power, no proof can be stronger than that it suspends hunger and enables the laboring man to pursue his work for hours after he would be otherwise unable. That it would soon become a most valuable article of diet among the laboring classes, and on ship-board particularly, if once brought into use, there can be no doubt. The coffee-tree can be grown to advantage for the leaf in the lowlands of every tropical country where the soil is sufficiently fertile, whilst it requires soil and climate to produce the fruit.

"Nothing appears in the *Free Press* on the mode of its

preparation by Dr. Gardner, but I should think if roasted and pulverized and packed in air-tight cases like tea, it would retain its strength and bear transporting to every part of the world; and as it soon fixes itself more strongly than either tea or coffee in the taste, it would soon become a more absolute necessary of life than either of those articles. In fact, I am acquainted with no tropical production capable of being rendered so great a blessing to mankind as the coffe leaf; and as it would tend materially to the desuetude of ardent spirits and strong drinks, its introduction ought to have the support of every friend to the moral and material welfare of society."—*Amer. Jour. of Pharmacy.*

Encysted Calculus, Severe Symptoms of Stone in the Bladder for Several Years; Imperfect Evidence of the presence of the Calculus. Death. Cyst of the Bladder containing the Foreign Body.—There is a rule respecting the operation of lithotomy to which operators genarally adhere—viz., never to cut for stone unless the presence of the calculus shall have been distinctly made out by percussion with the staff a few moments before the operation. This is an excellent rule, and it may be surmised that more than once both surgeon and patient have been saved the annoyance of a useless operation.

The case which we have this day to bring before our readers offers a beautiful illustration of the soundness of the above-mentioned practical caution handed down to us by our forefathers; for the post-mortem examination showed very plainly that, although there was really a stone in the bladder, none of the means at our command could have accomplished its extraction, even with a very large incision.

We happen to have witnessed two operations of lithotomy in charitable institutions of this metropolis, where no stone could be found; and it must be confessed, that the rule just alluded to was not, in in these instances, strictly followed. But it is, on the other hand, quite certain that the evidence by percussion is sometimes extremely faint; and it may happen that a surgeon will hear the sound, whilst his colleagues cannot perceive it. The course to

be pursued in such a case is not easly settled, but we may cite, as an encouragement for operating, a case sometime ago under the care of Mr. Le Gros Clark, at St. Thomas's Hospital. The little boy (about six years of age) had on a previous occasion been removed from the theatre without being operated upon, because no stone could be heard. When the patient was put upon the table for the second time, some of the surgeons present heard the stone, and others not. Mr. Clark was among the former; Lithotomy was performed, and Mr. Clark had the satisfaction of removing, by means of the scoop, a stone of the size of a pea.

We recollect an operation of lithotomy, performed a few months ago at Guy's Hospital, by Mr. Hilton, in which it appeared, when the forceps had been used for some little time, that the bladder contained no stone, although percussion had yielded the usual sound. But by dint of perseverance and well-directed efforts, Mr. Hilton succeeded in dislodging a moderately large calculus from above the pubes, in which locality it was firmly held by the muscular fibres of the bladder, or a small cyst.

Mr. Cock, at the same hospital had, on another occasion, a great deal of trouble in extracting a mulberry calculus, which was firmly grasped by the vesical mucous membrane and muscular coat, the anfractuosities of the stone being larger and more prominent than usual.

In Paris, M. Roux perfomed, some years ago, the lateral operation, in a case where the stone was distinctly heard; but no calculus could be found, and it was supposed that the foreign body was encysted somewhere behind the pubes. At all events some advantage might be derived from using the stethoscope more frequently when there is any donbt respecting the diagnosis, as advised by Skoda in the work lately translated by Dr. Markham. Still it will be seen by the following case, (for the details of which we are indebted to Mr Gamgee, house-surgeon to the hospital,) that even the sound yielded by the instrument striking against the calculus may deceive, and that the stone may be grasped in such a manner as to preclude the possibility of extraction.

John W—, aged forty-eight, was admitted Jan. 10, 1850, under the care of Mr. Arnott. The patient had left the army two years previously, was married, and of regular

habits. No malignant disease can be traced in the family. The patient suffered from inflammation of the testicle about twenty-two years before the date of this admission, and was six or seven years afterwards tapped for hydrocele; he also had gonorrhœa many years back, for which he used no injections. For two years and a half preceding his application at the hospital, the patient had very severe symptoms of irritated bladder, with occasional passage of blood; and on admission, the symptoms were *bonâ fide* those of vesical calculus. A week before the man was admitted, he passed, without pain, a small stone, about the size of a pea. A few days before he came into the house, five ounces of water were injected into the bladder; No. 9 sound was introduced; and on pushing it well into the viscus, the convex portion seemed to touch something rough; and on turning the point suddenly in the direction of the supposed foreign body, a distinct chink was produced. The urine was fœtid, turbid, pale, and slightly acid, and threw down a mucous deposit after standing. Under the microscope, phosphatic crystals, some pus, ann mucous corpuscles, were seen.

Mr. Arnott ordered one ounce and a half of infusion of pareira to be taken three times a day, and ten grains of Dover's powder at night. The symptoms for the next fourteen days became rather aggravated, notwithstanding injections of warm water and decoction of poppies into the bladder, and in spite of pretty large doses of opium, hyoscyamus, and nitric acid. The patient used to pass urine about fifteen times in the night.

The symptoms of stone became more and more urgent; the bladder was therefore examined by Mr. Arnott, Mr. Quinn, and Mr. Erichsen. The sound could not be turned to the right side so freely as to the left, this circumstance giving the idea of the existence of some growth on that side; greater pain was also experienced when the part was touched, but no stone could be felt, though the bladder was carefully explored. On pushing the sound well in, and turning it to the right side, the instrument did not seem to go beyond the projection just mentioned, and seemed to reach the point where a chink was before obtained. All medicines were left off, except opium enemata.

Twelve days after this, (five weeks from the admis-

sion,) the man, whose symptoms varied but little, was sounded again; the difficulty of turning the instrument to the right side seemed to be rather increased, but some allowance was made in consequence of there being very little water in the bladder; the instrument, when left to itself, had a direction to the left side.

Six days afterwards, the patient was sounded for the third time, with three ounces of water in the bladder; great suffering was experienced, but no stone felt.

Four days subsequently to this examination, chloroform was given, and the patient taken into the theatre. Three ounces of fluid were thrown into the bladder; but as no stone could be felt, the projected operation was not performed.

Up to the 22nd of March, (ten weeks after admission,) the bladder remained irritable, and the urine alkaline, though the patient was well supported with brandy, and took benzoic acid.

At this period small doses of copaiba were ordered; the urine thereupon became less alkaline, and the mucous deposit diminished considerably.

On the 30th of March, a week after this change of medicines, the patient only rose four times in the night, whilst a short time previously he used to be disturbed more than thirty times from his sleep.

Fifteen weeks after admission, April 18, 1850, he was so far improved that he wished to be discharged. The man presented himself several times at this hospital between the latter date and his finally entering the house in November, 1852. Mr. Erichsen sounded him repeatedly without success, except on one occasion, in the earlier part of the summer of 1851, when the stone was distinctly struck by the instrument.

The great frequency and pain of micturition caused great debility; and about one month before death, pleurisy was diagnosed on the left side and treated by blisters. Four days before the patient's death, a tumour nearly as large as the first showed itself on the left side of the thorax towards the lower part. It became a question whether this distinctly fluctuating tumour was due to escape of fluid from the chest. A puncture was made into it, and exit given to rather more than half a pint of very fœtid, brownish pus.

After this period the patient gradually sunk, and died Dec. 6, 1852. It is important to notice that the man was treated a few months before his second and last admission for a slight attack of jaundice.

The post-mortem examination was conducted by Mr. Gamgee, and took place thirty-three hours after death. On opening the thorax, the left pleura was found to contain 136 ounces of turbid serosity, with a few flocculi floating in it; the right pleura contained about a pint and a half of the same fluid. The lungs were studded at the apices by tubercles of the size of small peas, and the organs adherent to the parietal pleuræ. An incision being accidentally made into the upper portion of the pericardium, thin pus exuded from that bag. The latter was now cut carefully open anteriorly, and was found to contain sixteen ounces of purulent matter. Pus was seen exuding through the lower portion of the pericardium, and also through the diaphragm, about three inches behind the costal margin. The aperture allowing of this escape was of a circular shape, and capable of admitting the end of a goose-quill; the margin measured about a line in thickness, and was of a deep red colour. The whole surface of the heart was roughened by deposits of lymph, but the internal aspect of the organ was normal. No pathological change was noticed in the kidneys, save a very marked congestion. On the right lobe of the liver were seen several inequalities, resulting from contractions after deposition of lymph. The external margin of the left lobe was completely adherent to the false ribs, and in this locality a large cavity was observed, capable of admitting a good-sized fœtal head. This cyst contained pus, and communicated by an aperture, about the size of a goose-quill, with the pericardium, and also with the integuments by an oblique opening at the edge of the false ribs, where the abscess had pointed, and had been opened.

The bladder, upon which organ much interest was centred, was carefully dissected, and presented the following appearances:—Prostate gland healthy; bladder proportionally small; at the upper part, and on the right side, is a tumour projecting directly outwards, about the size of a pullet's egg. In a groove on the posterior portion of this protuberance, the right ureter is seen coursing. On manipulating this tumour, it is felt to contain calculous

matter; and on cutting into the swelling from without inwards, two whitish, slightly roughened stones are found. In this locality, the coats of the bladder are at least three times the thickness of the other parts of the organ. The latter was now opened by a longitudinal incision; and on examining the mucous surface, a small aperture, capable of containing the extremity of a middle-sized catheter, was obseved. This opening was situated about half an inch above the point of entrance of the right ureter, was perfectly circular in shape, and led into the cavity above described, which contained two calculi. On carefully examining this tumour, it seemed manifest that it was formed by a kind of cyst attached to, and communicating with, the bladder, which cyst had perhaps gradually taken its development around the calculi, and grown outwards, becoming subsequently considerably thickened.

It should now be observed, that the aperture within the bladder was just large enough to admit the extremity of the staff, and hence arose the distinct sound which had repeatedly been heard on the introduction of the instrument. When no chink could be produced, on the day when the patient was sounded in the theatre, as also on other occassions when no evidence of stone could be made out, it is very probable that the instrument either did not exactly come in contact with the small aperture communicating with the cyst, or that the opening was momentarily covered with mucus. It is evident that the patient might have been cut for stone, and no calculus been found, if, on the day he was brought to the theatre, the instrument had just struck on the quarter of an inch square of calculus surface which the cyst left uncovered; so that the rule which we mentioned in our introductory remarks would, in this instance, have failed to prevent a useless operation.

Besides the view which we just took of the formation of the cyst, it might be supposed that the latter existed originally or congenitally in the bladder, and that the calculi were the consequence of the stagnation of the urine in the pouch. By the post-mortem examination, it is now also clear why irritable bladder was the principal symptom presented by the patient. No foreign body was rolling in the vesical cavity, no stone stopped up the urethra suddenly, and therefore the main cause of distress was

the presence of the calculus, seemingly in the coats of the bladder, constantly irritating and dragging the organ. Nor did the stagnation of urine in the cyst, and its ever recurring decomposition, as well as the imperfect action of the coats of the bladder, contribute in a small degree to the patient's distress.

The pathological changes discovered in the bladder, taken in connexion with the symptoms and the difficult diagnosis, are of so interesting a nature, and have taken up so much of our space that we hardly have room to allude to the hepatic abscess which, in this patient, opened into the pericardium. The course here taken by the purulent matter is of a rather unusual kind; and it is somewhat strange, considering the quantity of pus which was found in the pericardium, that death did not occur more suddenly, the more so as one of the pleural sacs contained a large amount of serum. There may have been som connexion between the attack of jaundice and the hepatic abscess which subsequently formed, but the data are not sufficiently accurate to permit of any speculation on the subject.—*London Lancet.*

Color-Blindness.—Dr. Wilson, of Edinburgh, in a paper in the *Athenæum*, refers to this subject as having an important bearing on the system of railway signalling by colors' especially by red and green danger signals. This affection ("Daltonism," *Cromato pseudopsis, Achromatopsia*, etc.,) Dr. Wilson considers in three important practical relations; 1. That the affection is much more prevalent than is generally imagined. 2. That red and green, the colors used for danger signals on our railways, are exactly those which are most frequently confounded with each other by the subjects of color-blindness. 3. That color-blindness implies not merely a confusion in distinguishing between two or more colors, but, at least in many cases, an imperfect appreciation or feeble hold of color altogether as a quality of bodies. Prevost says, that color-blindness occurs in one male among twenty. Seebeck (in Poggendorff, Annalen, xliii. 177) found five cases among forty youths in Berlin. Professor Kelland, of the University of Edinburgh, has this session found three examples among 150 students. In four of the cases

which had come under Dr. Wilson's notice, none of them could be trusted to distinguish a red signal from a green one, and there was not only false vision of colors, but, in many instances, total color-blindness—so that the subjects of it doubted as to all colors, and would not swear in a court of justice as to any color. These facts, in connection with the continually augmenting number of railway accidents occuring, must show the imperative necessity of strict examination being instituted as to the perfect vision of all railway servants.—*Med. Gazette.*

A brief Summary of my Experience with the Veratrum Viride. By John S. Wilson, M. D., of Airmount, Clarke county, Alabama.—It is not my design to give a minute and tedious detail of cases; I will only mention some of the diseases in which I have used the medicine under consideration—the mode of administration, and the result, together with such general remarks as may be suggested to me in common with each case.

At the beginning of the present year, I succeeded in obtaining some of the root of the veratrum viride, after several ineffectual efforts: from this I made a tincture, according to the directions of Dr. Norwood, by adding ½ lb. of the root to 16 oz. of undiluted alcohol. This tincture I have used in the following diseases—viz: Pneumonia, pleuro-pneumonia, bronchitis, broncho-pneumonia, pericarditis, cerebro-spinal meningitis, epileptiform convulsions, acute rheumatism, general dropsy; and in fevers—typhoid, remittent and puerperal. Of the thoracic diseases, sixteen were pulmonary and one cardiac: of meningitis, convulsions, acute rheumatism, and dropsy, there was one case each: of the fevers, three were typhoid, one puerpal, and several remittent. Of the sixteen pulmonary cases, all recovered save one, and this was complicated by the supervention of acute rheumatism. The case of pericarditis had resulted in effusion, before it was put under treatment, and terminated fatally: still the veratrum exerted its wonted control over the pulse, reducing its frequency from 140 to 60. The case of cerebro-spinal meningitis was also fatal; but as in the other case, the remedy exerted a marked control over the pulse, and for

a while there was a flattering amelioration of all the symptoms.

The subject of the convulsions was an anemic boy of 10 or 12 years of age; the spasms were very frequent and extremely severe, every paroxysm apparently putting the life of the patient in imminent danger: the veratrum, to reduce the frequency of the pulse, and ether inhalations, to quiet the spasms, were the remedies almost exclusively relied upon; and these indications were fulfilled in the happiest possible manner, snatching the little sufferer from the very jaws of death.

The case of rheumatism occurred in a little boy about 5 years of age; it attacked the knee joint, was very severe, and was treated principally with colchicum and veratrum; but the former medicine did not succeed well until the latter was added to prescription. Under this treatment the disease was subdued in less than six days, instead of running "six weeks," according to the dictum of Dr. Warren.

The case of dropsy was fatal, the patient being moribund when the veratrum was commenced.

We now come to that interesting class of diseases—fevers. Of the three typhoid cases, one terminated favorably in twelve days; but the early convalescence in this case was not attributed to the veratrum, as this was not made a principal remedy, and as the fever was unusually mild. The other two cases were severe and protracted, one continuing three and the other six weeks. Having much confidence in the veratrum, it was relied upon almost exclusively in the treatment of these two cases, the prescriptions being complicated as little as possible. In the case that terminated in convalescence in three weeks, slight ptyalism supervened, from the use of small doses of calomel in conjunction with the veratrum.

The general plan of treatment which I have adopted in typhoid fever is to give quinine freely for the first few days, or until the typhoid symptoms are fully developed; I then resort to small doses of calomel and the veratrum, experience having taught me that nothing can be gained by pushing the quinine, *after the disease has become distinctly typhoid.* In giving the veratrum, I generally prescribe 4 gtt. every three hours, for an adult, to be increased 1 gtt. each dose, until a decided effect is produced, either

upon the pulse or stomach. I have never been able to carry the dose higher than 14 gtt., and seldom higher than 8 gtt. ; and I have even known 2 gtt. to produce a very manifest effect on the pulse of a little girl three years of age. I have never known it to fail in reducing the frequency of the pulse when it produced nausea, but I have frequently succeeded in reducing the pulse, without any nausea, by beginning with very small doses, and increasing gradually.

Having mentioned the disease in which I have used the veratrum, and the mode of administration, I conclude with a few general remarks

It will be seen by reference to the figures given, that my experience with it has been more extended in pulmonary diseases than in any other class—sufficiently so, I think to speak with some degree of confidence. As already stated, only one of those cases proved fatal, and that was complicated. Some of the cases were severe and alarming, as much so as any I have ever seen ; and one of the cases of pneumonia was accompanied with active pulmonary hemorrhage. With these facts before me, I can but express a very favorable opinion of the veratrum in pneumonia and bronchitis. Before the introduction of this remedy, I treated the diseases mentioned with antimonials, and generally with success ; but every physician of experience knows that pneumonia is not unfrequently complicated with a degree of intestinal irritation which forbids the use of tart. emetic on the Rasorian plan, or, indeed, in any way capable of subduing the pulmonary inflammation : and even where this intestinal complication is not an essential feature of the disease, it is well known that the free use of tart antimony will produce it, together with other grave, and even fatal symptoms. The veratrum is not obnoxious to those objections ; and having in addition to the important negative advantage, the positive one of being more prompt and certain in its effects, than the antimony, I think it entitled to the preference ; and I think it possible that time and experience will confirm its claims, and that it will be recognized as the great desideratum in the treatment of pulmonary diseases. In this class of diseases I have found it to act promptly, not only as a sedative, but also as an expectorant and anodyne ; and I would here remark, that I consider its pri-

mary acton, that which gives it its great value, sedative —while its secondary effects are expectorant, emetic, sudorific, and anodyne.

As to its use in fever, I have but few more words, as it is my design to confine myself mostly to facts, and as my experience with it in this class of diseases has not been extensive. As before intimated, the veratrum was made the principal remedy in the treatment of two of the typhoid cases—no other remedies being used except blisters to the abdomen, cold sponging, spts. mindereri, and in one case small doses of calomel. If I may venture an opinion as to the virtues of veratrum in typhoid fever, from its effects in those two cases, it is this: Its powers in abbreviating the fever are doubtful; but its control over the pulse, the reduction of excessive arterial action, and the consequent mitigation of all the more distressing symptoms, are, comparatively, certain—far more so than any remedy previously in use. But, while I express myself thus favorably, I do not think the veratrum is applicable to *all* cases of typhoid fever; for I would be averse to its use in cases attended with extreme debility and great prostration of the vital powers: If I were to resort to it at all in cases of this kind, it would be in doses too small to produce nausea and prostration, observation having taught me that the patient must have a considerable degree of recuperative energy to withstand its depressing effects, unless it is used with great caution.—*South. Med. and Surg. Jour.*

Duty Of Medical Men.—Dr. Todd, in his farewell address, on resigning his professorship, made the following remarks:—

"It appears to me, that when a man proposes to devote himself to the practice of an honorable profession, he has a twofold duty to perform; first, to fit himself to the utmost of his ability for the practical duties of that profession, and, secondly, having done so, it is incumbent on him to divest himself, as far as possible, of every engagement which may interfere with his bending his thoughts and attention to the various anxious, difficult, and often perplexing questions which are continually arising

in the course of his professional practice. Every member of a liberal profession should keep it constantly in view that he exercises his calling not simply for his own personal benefit, but for the public good, and for the good of his profession at large. So every practising physician or surgeon, whether the sphere of his labor be within wide or narrow limits, should bear in mind that in successful application of his art, by fair, honorable, and truthful means, is involved the repute and estimation in which his profession is held by the public at large. Let each of us act under the feeling, that to himself specially is committed the keeping of the honorable character and the scientific credit of our common profession, and he will have the strongest motives, not only to eschew everything that savors of charlatanical pretence, but to seek for and insure the highest means of moral and intellectual culture." *Med. Gazette.*

Stomatitis Materni.—" Has this disease ever existed when the symptoms of *general* disorder were not accompanied by it, traceable to either retention or suppression of bile?" In the last 15 years I have not found it necessary to take the child from the breast, and my practice has been based on a negative answer to this question. I have witnessed two post obit. examinations of patients, who labored, at time of death, under this disease, in each the gall bladder and duct were clogged with calculi and thickend bile. In the living patients I have observed, in almost every instance, 1st, an anemic condition; 2nd, acidity of stomach, 3d, as a consequence of the last, urinary deposits, *invariably relieved by the free use of alkalies*; 4th, the alvine evacuations either hard or black, denoting the absence of the natural and accustomed stimulus to the bowels, or clay, colored with a tendency to diarrhœa, showing both a want of biliary presence, and the existence of fermentations, which never (I think) occurs when the proper proportion of bile is present

The presence of anæmia I have not found necessary to the existence of the disease, but if present, it greatly adds to the chances of continuance. Indeed, it often produces the very state of things, on which, I think, the disease de-

pends. In an anæmic condition it is difficult for the system to perform all of its *usual* functions, and at the same time support the new one now set up—the support of the infant, either in embryo or by lactation. For this support a *new* draft is made upon the energies of the system, and this draft is often made upon some *one* organ, instead of beeing distributed equally on the whole; that one must, of course, lose much of its energy; and, should it be the liver, we have want of its secretion; as a consequence, we have fermentation and acidity of stomach and bowels with its sequents, amongst which is ulceration of certain of the mucous membranes; and, to the existence of this condition of the membranes, neither pregnancy nor lactation are *necessary*, as it will be found under many circumstances in the absence of both these, though *never* under my observation without the previous withdrawal of the accustomed supply of bile. The only difference to be found in the disease under these different circumstances is, that during lactation or pregnancy it is more obstinate in consequence of the draft upon the energies of the system being continuous, and consequently renewing the disease as often as the remedies for its relief are withheld, or until the vital engeries are increased by tonics, or otherwise sufficiently to meet the new demand without the special draft.—*Ibid.*

THE WESTERN JOURNAL.

THIRD SERIES.—VOL. XII.—No. II.

LOUISVILLE, AUGUST, 1853.

TRIBUTE TO DR. CALDWELL.

The death of Dr. Caldwell is announced in another part of this Journal. The following proceedings growing out of that event will be read with interest:

At a meeting of the physicians of Louisville held on the 12th July, 1853. Dr. J. W. Knight was called to the chair, and Dr. B. I. Raphael appointed secretary.

On motion of Dr. J. J. Mathews, a committee of three was appointed to draft resolutions expressive of our feelings in reference to the death of our venerable friend and fellow-citizen, Dr. Chas. Caldwell.

The following gentlemen were appointed by the chair on said committee: Dr. J. J. Mathews, Dr. T. W. Colescott, and Dr. H. Chenoweth.

The committee having retired, returned in a short time and presented the following report, which was unanimously adopted:

It having pleased an all-wise Providence to call from the scene of his earthly labors, the venerable Dr. Chas. Caldwell, the most widely known and the most eminent member of the medical profession in the Union, endeared to us in the three fold relation of neighbor, friend, and teacher, by the blended dignity and courtliness of his manner, the purity of his life, his profound and varied learning, and his indefatigable labors for the

space of two generations in the cause of science and humanity; therefore—

Resolved. That we have received with profound regret the intelligence of the death of Dr. Caldwell.

Resolved, That, in the death of Dr. Caldwell, the American Medical profession has lost one who, while living, most illustrated its annals by his talents and adorned it by his virtues; medical science one of its oldest and most ardent votaries and medical philosophy the most distinguished and successful interpreter that our country has yet produced.

Resolved, That with a profound sense of the vast services rendered by Dr. Caldwell to the science of medicine, and the great interests of society, we will not cease to cherish and to guard his memory with affectionate respect and admiration.

Resolved, That we tender to the bereaved family of the deceased our warmest sympathy, and that the secretary be directed to transmit to them a copy of these resolutions.

On motion it was—

Resolved, That Drs. Raphael, Hardin, and Ronald be constituted a committee to select a suitable person to deliver a eulogy on the deceased.

It was further—

Resolved, That these proceedings be published in the daily papers of the city, in the Western Journal of Medicine and Surgery, and the Transylvania Journal of Medicine.

On motion, the meeting adjourned *sine die.*

J. W. Knight, Chairman.

B. I. Raphael, Secretary.

THE FISHES OF THE UNITED STATES.

We lay the following letter before our readers in the hope that among those in Kentucky there may be some who will be disposed to co-operate in the work alluded to by Prof. Agassiz. It is a work in which every lover of science in our country must feel an interest. We trust, by means of this circular, that we shall be able to collect some information useful to the great naturalist; and have to beg the favor of any of our friends who can give assistance themselves in collecting the fishes of Kentucky, or can refer us to others who will engage in it, to communicate with us, or directly with Prof. Agassiz, on the subject.

Cambridge, 14th July, 1853

Dear Sir:

The lively interest you have taken for so many years in the progress of science in this country may be my apology for asking your co-operation in an investigation in which I am deeply engaged.

Perhaps I may also trust to your kindness towards me, and assure you I shall value highly any assistance you may be willing to render me on this occasion. For years I have been collecting materials for a general Natural History of the Fishes of the United States, in which I should like not only to give as complete a description as can be drawn up from preserved specimens of all the species found in our waters, but also a minute account of their geographical distribution, which it is high time to study now, before increased navigation has modified it extensively. It becomes, therefore, necessary that I should obtain separate collections of the fishes, shells, and crawfishes, which I mean to include all in my investigations, from every watercourse in the United States. This is such a gigantic undertaking for a private gentleman, that I trust scientific men may assist me in different parts of the country, whilst I should be happy to repay any expense they incur in making such collections, if I only can have them brought together.

In this respect, the State of Kentucky is one of the most important of the Union, not only on account of the many rivers which pass through its territory, but also because it is one of the few States the fishes of which have been described by former observers, especially by Rafinesque in its Ichthyologia Ohiensis, so that a special knowledge of all his original types is a matter of primary importance for any one, who would compare with each other, the fishes of the different rivers of the West. Would it be asking too much of you to inform me of the persons in different parts of your State, who might be applied to, to make such collections for me. From the Ohio river I should want three distinct collections, one made anywhere between the junction of the Big Sandy river with the Ohio at Maysville, another about Louisville, and a third anywhere between the mouth of Green river and that of the Cumberland rivrer. From the tributaries of the Ohio I want a collection from the Big Sandy river, another from the Licking, another from the Kentucky river, even two if possible, one from its headwaters and another from its lower course above its junction with the Ohio, another from Salt river, if possible two of Green river, one above and another below the Mammoth Cave, one from the headwaters of the Cumberland and another from its lower course, as also one from the lower course of the Tennessee river. Indeed, it might be desirable to obtain even more collections from all the different creeks forming by their reunion these different rivers, were it possible to obtain a sufficient corps of volunteers for such a purpose. However, there is no sort of difficulty in

making such collections; all that is required is a keg of alcohol, one head of which is taken off and used as a cover, until the keg is full. In packing it to send off, it is only necessary to see that the fishes are placed close together in layers like herrings, filling the barrel to the very rim, to prevent any jarring and rubbing of its contents; perhaps an occasional layer of paper or cotton cloth between the layers of fishes will assist in keeping them in good condition. After heading again the keg, it is to be filled through the bung hole, or a hole in one head with alcohol of about 80 spec. grav., or rather somewhat stronger. Too much alcohol is injurious, inasmuch as it contracts the fishes and renders them stiff. The alcohol first used to receive the specimens may be employed to pack them, after strengthening it somewhat with a purer article. Could you not make an appeal in my behalf to your people, and mention to me the names of gentlemen living upon your different watercourses in appropriate situations, who might be induced to collect in that way, to aid me in the preparation of a work, which I should like to render so valuable that it would appear as a national standard work. You see that it would require no knowledge of the fishes or shells themselves to make such collections, but only good will, and an opportunity to ask from fishermen specimens of all the fishes of their respective localities. I have found, also, that boys take readily hold of such matters, indeed there are many species which may better be obtained from them than from fishermen, who generally value only the marketable kinds, whilst we naturalists esteem a minnow as highly as a trout, at least for our collections. Of the small kinds of fishes I should like as many as twelve specimens, four to six of those of medium size, and one or two of the larger ones

I would naturally ask first, whether there is not in Louisville some young naturalist among your students, who would take hold of this subject, and make a thorough collection of all the fishes of your immediate vicinity, or could you see yourself to it? I never regretted more than now that circumstances have not yet allowed me to visit your State and to make a stay in Louisville; but I trust it will be one of my first excursions.

EXPERIMENTAL INVESTIGATION OF TABLE-MOVING.

Our readers cannot be unaware of the excitement which prevails, not only in this country, but in London and Paris, on the subject of certain phenomena styled "table-turning," "table-tipping," &c., and which have been erroneously referred to electricity, magnetism, a spiritual, or diabolical agency, or some new force in nature not yet understood. The evil of it has already been very great, for as most people adopt the supernatural theory of the matter a morbid feeling has been created which, in not a few instances, has terminated in insanity. Prof. Faraday, one of the most eminent philosophers of our times, has engaged in an experimental investigation of the subject, and the results of his inquiry have been published in the *London Athenaeum*. They are so satisfactory, so perfectly conclusive, that we wish to give as wide a circulation to them as possible. Prof. F. prefaces the account of his experiments by saying—

"I have been greatly startled by the revelation which this purely physical subject has made of the condition of the public mind. No doubt, there are many persons who have formed a right judgment or used a cautious reserve—for I know several such, and public communications have shown it to be so; but their number is almost as nothing to the great body who have believed and borne testimony, as I think, in the cause of error. I do not here refer to the distinction of those who agree with me and those who differ. By the great body I mean such as reject all consideration of the equality of cause and effect—who refer the results to electricity and magnetism, yet know nothing of the laws of these forces—or to attraction, yet show no phenomena of pure attractive power—or to the rotation of the earth, as if the earth revolved round the leg of a table; or to some unrecognized physical force, without inquiring whether the known forces are not sufficient—or who even refer them to diabolical or supernatural agency, rather than suspend their judgment, or acknowledge to themselves that they are not learned enough in these matters to decide on the nature of the action. I think the system of education that could leave the mental condition of the public body in the state in which this subject has found it must have been greatly deficient in some very important principle."

The object which Prof. F. had in view in this inquiry, he says, was not to satisfy himself, for he was convinced as to the true explanation of the phenomena by the reports of the table-turners themselves, "but that he might be prepared to give a strong opinion, founded on facts," to the many who applied to him for it. The methods which he adopted were precisely such as he would have followed in any other physical investigation. The parties with whom he worked were very honorable, very clear in their intentions, successful table-movers, very desirous of suc-

ceeding in establishing the existence of a pecular power, thoroughly candid, and very effectual." With him it was "a clear point that the table moves when the parties, though they strongly wish it, do not intend, and do not believe that they move it by ordinary mechanical power." The account they give is, that "the table draws their hands; that it moves first and they have to follow it; that sometimes it moves from under their hands." "Though," says Prof. F. "I believe the parties do not intend to move the table, but obtain the result by a *quasi* involuntary action, still I had no doubt of the influence of expectation upon their minds and through that upon the success or failure of their efforts." He proceeded to test this opinion, and so arranged his apparatus as to show any electricty that might be evolved, and at the same time any pressure that might be exerted by the table-mover. Not the slightest trace of electrical or magnetic effects was obtained," and no experiment "gave the slightest indication of any peculiar natural force. He adds: "

"No attractions, or repulsions, or signs of tangential power appeared —nor anything which could be referred to other than the mere mechanical pressure exerted inadvertently by the turner. I therefore proceeded to analyze this pressure, or that part of it exerted in a horizontal direction, doing so, in the first instance unawares to the party."

We omit his description of his apparatus, simply remarking that the arrangement was such as to be perfectly convincing to the table-mover himself whether or not he was using pressure. The result was that,

"On examination, it was easy to see by the displacement of the parts of the line, that the hand had moved further than the table, and that the latter had lagged behind—that the hand, in fact, had pushed the upper card to the left, and that the under cards and the table had followed and been dragged by it. In other similar cases when the table had not moved, still the upper card was found to have moved, showing that the hand had carried it in the expected direction. It was evident, therefore, that the table had not drawn the hand and person round, nor had it moved simultaneously with the hand. The hand had left all things under it behind, and the table evidently tended continually to keep the hand back."

He modified his apparatus, so as to show by an index whether the pressure exerted was downwards, or in a horizontal direction. Where he parties thought they were pressing downwards, the index proved that they were pressing horizontally.

"When shown that it was so, they were truly surprised; but when they lifted up their hands and immediately saw the index return to its normal position they were convinced. When they looked at the index

and could see for themselves whether they were pressing truly downward or obliquely so as to produce a resultant in the right or left-handed direction, then such an effect never took place. Several tried, for a long while together, and with the best will in the world; but no motion, right or left, of the table, or hand, or anything else, occured."

The effect of this test apparatus was to correct the mind of the table-movers. When they saw the index, it remained stationary; when hidden from their view, it moved under the pressure of their hands—a curious example of self-deception. Prof. F. continues—

"As soon as the index is placed before the most earnest, and they perceive—as in my presence they have always done—that it tells truly whether they are pressing downward only or obliquely, then all effects of table-turning cease, even though the parties persevere, earnestly desiring motion, till they become weary and worn out. No prompting or checking of the hands is needed—*the power is gone*; and this only because the parties are made conscious of what they are really doing mechanically and so are unable unwittingly to deceive themselves."

Just as there are but few people, comparatively, who can be "mesmerized," so there is but here and there an individual capable of this self-deception, and consequently there are not many so called "*media*." It is only those able to impose upon themselves who can become "table-turners."

By the use of this instrument, says Prof. F., the mind of the table mover is instructed.

"The involuntary or *quasi* involuntaary mention is checked in the commencement, and therefore never rises up to the degree needful to move the table, or even permanently the index itself. No one can suppose that looking at the index can in any way intefere with the transfer of electricity or any other power from the hand to the board under it, or to the table. If the board tends to move, it may do so; the index does not confine it; and if the table tends to move, there is no reason why it should not. If both were influenced by any power to move together they may do so—as they did indeed when the apparatus was tied, and the mind and muscles left unwatched and unchecked."

We have not a doubt in the world, that this is the true explanation of these movements, which have seemed so mysterious, and have come nigh running crazy whole communities. We have constantly referred them to *muscular contractions exerted unconsiciously* by the individual, the same that execute the "Spiritual Writings," which we noticed some time ago and accounted for upon this principle. The philosphy of the whole matter is given in the last edition of Carpenters Physiology, to which we would refer for a full exposition of the subject. Prof. Faraday owns in conclusion, that he "is a little ashamed of his communication," so

ceeding in establishing the existence of a peculiar power, thoroughly candid, and very effectual." With him it was "a clear point that the table moves when the parties, though they strongly wish it, do not intend, and do not believe that they move it by ordinary mechanical power." The account they give is, that "the table draws their hands; that it moves first and they have to follow it; that sometimes it moves from under their hands." "Though," says Prof. F. "I believe the parties do not intend to move the table, but obtain the result by a *quasi* involuntary action, still I had no doubt of the influence of expectation upon their minds and through that upon the success or failure of their efforts." He proceeded to test this opinion, and so arranged his apparatus as to show any electricty that might be evolved, and at the same time any pressure that might be exerted by the table-mover. Not the slightest trace of electrical or magnetic effects was obtained," and no experiment "gave the slightest indication of any peculiar natural force. He adds: "

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the reason that "he thinks, in the present age and in the part of the country" for which he wrote, " it ought not to have been required." We are glad that he took the subject up, his authority being the highest in all questions relating to physical science; but at the same time, we have no idea that his solution of the mystery will be satisfactory to the masses, or that it will suddenly cure the delusion. It must have its run; but fortunately, the malady is self-limited.

CIRCULAR MEDICAL ASSOCIATION.

The Treasurer of the American Medical Association makes the following statements, which we hope will arrest the attention of all our readers. More subscribers are wanted to the Transactions of the Association. We solicit the co-operation of every physician who desires the advancement of his profession. These Transactions are of great interest and ought to be in every medical library in our country

PHILADELPHIA, *June* 14, 1853.

To the Medical Profession:—In conformity with a resolution of the American Medical Association it becomes my duty to inform you of the decision of the Committee of Publication in regard to the price at which the forthcoming volume of Transactions will be furnished to the members of the Association and to others.

The price of *single copies* has been fixed by the Association at *five dollars*. Any *individual* desiring two copies, will be furnished with them upon his remitting to the Treasurer *nine dollars*.

Societies, Associations, and Institutions requiring *six* copies for the use of their members, will be supplied with them on remitting to the Treasurer *twenty five dollars*, or they will be supplied with twenty-five copies on remitting *seventy five dollars*.

Those who wish volumes 5 and 6, may obtain them by remitting *eight dollars*.

In consequence of the numerous illustrations, many of them highly finished colored lithographs, with which the forthcoming volume will be accompanied, its cost will considerably exceed that of either of those previously issued by the Association. To defray the cost of its publication, at least *fifteen hundred dollars*, in addition to the amount already received by the Treasurer, will be required

Your attention is called to the following resolution adopted at the last session of the Association.

" *Resolved*, That the Delegates from the several States be requested to appoint committees, who shall aid the Committee of Publication in procuring subscribers for, and in distributing the Annual Transactions of the Association." Respectfully, D. FRANCIS CONDIE,

Treasurer and Chairman Com. of Publication A. M. A.

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